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ON THE CONSERVATION OF SOLAR ENERGY.

BY DR. W. B. CARPENTER.

THE meeting of the Royal Society on March 2nd was rendered unusually interesting, first, by the admission of H.R.H. the Prince of Wales, as a Fellow of the Society; second, by a communication given by Prof. Huxley on the fungous origin of the "Salmon Disease," which is destroying large numbers of fish in the rivers of the South of Scotland and the North of England, from the Tay to the Conway; and last, but by no means least, by the exposition given by Dr. Siemens of an "idea" regarding the mode of maintenance of the Solar energy, which he has been for some time maturing, and has at last determined to submit to the criticism of the scientific world. Of this most ingenious and suggestive speculation, the following sketch will, I hope, prove as interesting to the readers of KNOWLEDGE, as Dr. Siemens's own admirable and more detailed statement of it was to the members of the large scientific gathering to which it was addressed.

In the first place, he reminded us of the enormous amount of heat which is constantly radiating from the Sun into space; this, according to the best measurements that have been made, being such as would be maintained for only thirty-six hours by the complete combustion (as in the most perfectly-constructed furnace) of a mass of the best coal equal to the Earth in bulk. Now, if the sun were surrounded by a solid sphere of a radius equal to the mean distance of the earth, the whole of this heat would be intercepted by it; but since the diameter of the earth, as seen from the sun, is only seventeen seconds, so that its surface is only 1–2,250,000,000th part of the whole area of such a sphere, only that proportion of the entire heat radiated from the sun will fall upon the earth. Supposing the aggregate of all the Planetary bodies to intercept ten times as much as the earth, the total amount of solar heat thus utilised will be only one part in 225,000,000 of the total radiated from the sun; the other 224,999,999 parts to all appearance going to waste—in other words, doing no work.

Now the mode in which this enormous supply is kept up has been in all ages a question of great interest; but only in modern times could any scientific solution of it be even attempted. Of course, Chemical action would be the first source that would occur to almost every one—radiation of heat from a fire being the nearest thing within our experience to the heating effect of the solar beams. But, putting aside other difficulties arising out of the revelations of the spectroscope, the ordinary chemical hypothesis is met by the objection, that the accumulation of the products of combustion on the surface of the sun would in time form a barrier against further action. And, supposing this barrier disposed of, it is obvious that the maintenance of this combustion must be attended with a continual *wasting-away* of the sun, at a rate which would make itself perceptible in the disturbance of planetary equilibrium, when the loss is estimated for long periods of time.

An opposite idea was suggested some years ago by Sir William Thomson: that of a continual rain of Meteorites upon the sun—the velocity they would acquire from its attraction causing them to impinge upon its surface with such force, as to generate a large amount of heat when their motion is checked. But here we are met by two difficulties: first, that of conceiving of any supply of meteorites that would be competent thus to keep up the amount of heat which we know to be always radiating from the sun; and secondly, the progressive *increase* in the bulk of the sun that would be produced by any adequate supply, disturbing the planetary equilibrium in the contrary sense to the preceding.

It has been supposed by Helmholtz, and accepted by many physicists on his authority, that the radiant energy of the sun is the result of a progressive *shrinkage* of his bulk and condensation of his substance. But the giving-out from his surface of the heat thus generated in his interior, could only be accomplished through some medium of much greater conductivity than is possessed by any material known to us; and on this process, again, a limit is obviously imposed, since a time would come when (as seems now the case with the Moon, and nearly so with the Earth, Venus, and Mars) the limit of consolidation would be reached.

Dr. Siemens, as every one knows, is the inventor of the regenerative furnace now coming into general use; in which a large proportion of the heat that ordinarily goes up the furnace-chimney, and runs to waste, is recovered from the products of combustion, carried back into the furnace, and made to do its proper work—thus obtaining an enormous advantage in economy of fuel. Mentally projecting this terrestrial experience into the realms of space, he was led to the conviction "that the prodigious and seemingly wanton dissipation of solar heat is unnecessary to satisfy accepted principles regarding the conservation of energy; but that it may be arrested and returned over and over again to the sun, in a manner somewhat analogous to the action of the heat-recuperator in the regenerative gas-furnace." The fundamental conditions of his hypothesis are three.

I. Everyone who has followed the recent progress of Celestial Physics, is aware of the increasing reasons which there are for regarding not only planetary, but stellar space as occupied by *matter* in a very attenuated condition; and Dr. Siemens starts with the assumption that this matter chiefly consists of hydrogen, oxygen, nitrogen, carbon, and their compounds (especially aqueous vapour and carbonic acid), besides solid material in the form of dust. The existence of oxygen, nitrogen, and carbon he considers to be indicated by the presence of those elements in our own atmosphere, to which (according to the molecular theory of gases) no such limit as was formerly assigned to it can now be admitted. We get a clue to the gaseous components

of what may be called the "atmosphere of space," from analysis of the gases locked-up in freshly-fallen meteorites, which sometimes "occlude" six times their own bulk. A recent analysis by Dr. Flight gave nearly 46 per cent. of the total as consisting of hydrogen, 32 per cent. of carbonic oxide, and 18 per cent. of nitrogen; and it seems clear that the hydrogen and carbonic oxide could not have been absorbed during the passage of the meteorite through our own atmosphere, but must have been brought in from the outside. Further proof that stellar space is filled with gaseous matter is furnished by spectrum-analysis; and the recent investigations of Dr. Huggins and others into the composition of the last great Comet showed it to contain very much the same gases with those contained in meteorites.

II. It was long since shown by Sir William Grove that water can be decomposed—or, in modern chemical language, that oxygen and hydrogen can be "dissociated"—by heat alone; and we know that the dissociation of the oxygen and carbon in carbonic acid, is effected by light, acting through certain vegetable substances. Now, according to the law of dissociation developed by Bunsen and Saint-Claire Deville, the point of dissociation of different compounds depends upon temperature on the one hand and pressure on the other; so that it is quite conceivable that when aqueous vapour is reduced to extreme tenuity, its dissociation may be effected by solar radiation at a comparatively low temperature. Some years ago Dr. Siemens tried some experiments on this point, the results of which were (so far as they went) confirmatory of this view. And his recent well-known experiments on the growth of plants under the electric light have satisfied him that, provided the source of the light give it off in sufficient *intensity*, the *quantity* required is very small. And he is thus led to suggest that all the radiant energy which is seemingly running to waste, is really doing work in dissociating the aqueous vapour and carbonic acid of the "space atmosphere," the carbon being thus made ready to unite with the nascent hydrogen into combustible hydrocarbons.

III. The third basis of Dr. Siemens's doctrine is the effect that will be produced by the rotation of the Sun around its axis, on the distribution of gases and vapours in its atmosphere. The tangential velocity of the sun at its equator being nearly four times that of our earth, an extension of the solar atmosphere must take place in the equatorial plane, to which (reviving an old hypothesis, and explaining away the objection raised to it by Laplace) Dr. Siemens attributes the "zodiacal light." Pressures being balanced all round, Dr. Siemens shows that the sun would be continually drawing hydrogen, hydrocarbons, and oxygen from the "space atmosphere" towards its polar surfaces, and be continually projecting outwards the products of their reunion, from the equatorial extension of its own atmosphere. During their gradual approach, they will pass from their condition of extreme attenuation and extreme cold to that of compression, accompanied with rise of temperature; until, on approaching the photosphere, they burst into flame, giving rise to a great development of heat, and themselves acquiring a temperature proportionate to the pressure they are sustaining. The result of their combustion will be aqueous vapour and carbonic oxide or carbonic acid, according to the sufficiency or insufficiency of the oxygen present to complete the combustion; and these products of combustion, yielding to the influence of centrifugal force, will flow towards the solar equator, and be thence projected into space.

In this manner a continual interchange of matter will be taking place between the sun and its "environment;" and as the Sun is constantly and rapidly moving through space, it will be continually traversing new portions of the "space-atmosphere," which, it is conceivable, may be so differently charged with the supplies of material, as to be more or less potent in maintaining the solar energy.

Such is a general outline of Dr. Siemens's most ingenious speculation, which, whatever may be its ultimate issue, must be accounted one of the highest and most brilliant flights that the "scientific imagination" has ever made. Such as desire a more detailed exposition of it—especially as to the changes which Dr. Siemens supposes to be always taking place on the surface of the sun itself—will find it in his paper, which will speedily appear in the "Proceedings of the Royal Society." Its publication will doubtless give rise to much discussion; and, whatever may be the ultimate fate of the doctrine as a physical theory, there can be no doubt that in the new direction which it will give to investigation, its promulgation will contribute in no small measure to the advance of science.

NOTES ON ROWING.

By an Old Club Captain.

LET us, in the first place, consider the conditions under which an ordinary lap-streaked inrigged (or half outrigged) boat should be rowed in order to get the best racing speed for a boat of that sort. I, of course, assume the rower to have mastered all the initial difficulties of his art, so as to be able to give his attention to the question of style. Well, in the first place we find that for racing purposes the great object is to adopt a style by which we may *maintain*, as far as possible, the velocity which can be readily enough communicated by a great short-lasting effort, and to do this with as little overwork as possible. Racing necessarily involves overwork, for no one who meant to row for two or three hours, or even for a single hour, would adopt a racing stroke, even for five minutes of the time. But the overwork in a race has to last over a good many minutes, and must be so distributed as to be most effective. The rower has, therefore, in racing to avoid, above all things, whatever would involve waste of power; and he very quickly finds that the most mischievous waste of power results if he suffer the extra speed communicated by his efforts to be lost more than of necessity it *must* be lost between the strokes. In other words, a given average of velocity is obtained with greater or less expenditure of force, according as the necessarily varying velocity of the boat ranges more or less above and below that average. Or we may put the matter this way (it is not without a purpose that we put it in both ways): The more uniform the velocity, the less the total expenditure of power to attain a given average rate of speed.

When we say that a rower soon finds this out, we mean that if he is attentive and apt he does so. As a matter of fact, the most successful oarsmen (in races) are those who, whether they know it or not, have practically found this out, and the rules for a good rowing style are based—as will presently appear—on this important principle. But we know that, apart from training and example, numbers of stout oarsmen would never attain a good rowing style, or at any rate a good racing style. So that we might probably have said more truly that nine-tenths of our rowing men would not of themselves discover this law, which comes out very gradually in rowing practice, even to the acuter rower, and is theoretically only to be established by somewhat difficult reasoning, based on recondite principles, partly dynamical, partly physical, and partly physiological.

But now notice how in the rules for rowing in the old-fashioned racing boats this principle shows itself.

Oarsmen were told in those days, and very soundly, to row in the following way :—A good reach forward was to be taken, and the water caught squarely by the oar, not by a pulling action of the arms, but by the action of the body and legs; the arms were to remain perfectly straight, acting only as "stretchers" until the body was a little past the perpendicular; then the stroke was to be finished by the combined action of the arms, body, and legs—the body slanting back, the hands drawn well in to the chest. The "recovery" followed, the body being thrown rapidly forward from the hips, the arms being at the same time extended, so that, the handle of the oar being thrust forward by both motions simultaneously, the blade passed with exceeding rapidity to the proper position for beginning the next stroke.

If we consider the dynamical effects of this action, we shall see how admirably suited they were to produce a motion as uniform as possible in the racing boats of those days. (Such directions are given in "Principles of Rowing by Oarsmen," somewhere about 1840, by Bob Coombes, who became champion in 1846, and by other excellent authorities of that time.) First, the strength was applied with gradually increasing effect from the beginning to the end of the stroke, so that there was no undue strain in increasing the motion of the boat from the velocity to which it had fallen during the "recovery" to its maximum just before the "feather." Then the work was carefully distributed between arms, legs, and body, the body and legs doing the work first, then the arms joining them to give that extra lift at the finish which was meant to counteract as much as possible the tendency to lag between the strokes,—so marked a characteristic of the old-fashioned racing boat. Lastly, that this tendency might have as little chance as possible to give the oarsmen extra or waste work, there was a very rapid recovery, so that the next stroke might begin under as favourable conditions as possible.

All these rules are admirable for the heavier class of boats, or for those which in old times were called racing boats. They served to obviate what, from our present point of view, may be called the great defects of those boats, their breadth of beam, and the (relative) clumsiness of their structure.

These rules were carefully enjoined at both the Universities; but they were more perfectly carried out at Cambridge than at Oxford. The sway back of the Cambridge crews and their rapid "recovery," were things to be marvelled at in some of the great races which preceded the introduction of light, outrigged racing boats. And those who adopted this system had their reward. Of six races rowed on the Thames in the old-fashioned craft, Cambridge won five. Not only did they win as a rule, but they often won in that hollow fashion which means that superior style has won the race, and not mere superiority of strength, or even of pluck (in both which, University crews are likely to be pretty evenly matched.) Cambridge won by a full minute in 1836, by a minute and three-quarters in 1839, by more than a minute in 1841, by half a minute in 1845, the last race rowed in the old-fashioned inrigged boats.

From 1846 to 1856 the University race was rowed in boats which had a sort of intermediate position between the heavy lap-streaked inrigged boat and the present light keelless outrigged craft. We should consider the boats used during those ten years quite unsuitable for racing purposes in our time. The old style of rowing suited them well enough—perhaps as well as the modern style; a style between the two would probably have suited them better than either. In the seven races between Oxford and Cambridge rowed in these earlier specimens of the outrigged racing boat, success was pretty equally divided between Oxford and Cambridge—counting one race won by Oxford on a foul as a real win (which it certainly would have been, Oxford showing the better speed), each University won three. But the Oxford wins were better, especially in the latter years. Cambridge won by two lengths in 1846, by four in 1849, by half-a-length in 1856 (when Cambridge had an exceptionally powerful crew). Oxford won by eight lengths* in 1852, by about five in 1854, and would probably have won the race of 1849 by many lengths, apart from the foul. However, six years are not enough to judge by.

So soon, however, as we turn to the races rowed since the introduction of the modern racing-boat in its present form (except as to sliding-seats), we find the University which had been almost always successful in long races with the heavy craft, and which had seemed able, very fairly, to hold its own in the keeled outriggers, beaten, not only in the great majority of races, but also by much the greater distances. Let us consider the twenty-five races which have been rowed between Oxford and Cambridge since 1857 :—

Of these twenty-five races, fifteen have been won by Oxford, nine by Cambridge, and one was a dead heat. If we count the race of 1859 as one which Cambridge would have won had not the Cambridge boat been half full of water at starting, we may put fourteen races to Oxford and ten to Cambridge; but then, in fairness, the dead heat of 1877 should be counted as an Oxford win.† The mishap to Thorley's outrigger in the Oxford boat in 1858 may be regarded as fairly matched by the accident to Dick's stretcher in the Cambridge boat in 1875. This disproportion is too great to be probably due to mere chance. But when we examine the circumstances under which the various races were lost and won, we find the existence of a determining cause still more clearly indicated. Take for this purpose the following table, in which, to eliminate as much as possible the effect of mere chance, all the races since the introduction of outrigged craft are considered :—

Oxford won		Cambridge won	
In 1852 by	8 lengths	In 1846 by	2 lengths
1854	5	1849	5
1857	11	1856	½
1861	14	1858	7
1862	8	1860	1
1863	13	1870	1½
1864	6	1871	1
1865	4	1872	2
1866	4	1873	3
1867	½	1874	2½
1868	4	1876	8
1869	3	1879	2½
1875	1[illegible]	...	...
1878	10	...	...
1880	3½	...	...
1881	3	...	...
Total	107 lengths.	Total	36 lengths.
Average per race ...	$7\frac{8}{15}$ lengths.	Average per race...	3 lengths

* The number of lengths corresponding to any given number of seconds by which the race was won, may be obtained by regarding $6\frac{1}{2}$ *lengths* as equivalent to as many *seconds* as the race itself lasted *minutes*.

† The reader will understand that we are only regarding either race as affecting our opinion of Oxford and Cambridge style. The race of 1859 was unquestionably an Oxford win, though every one who knows the circumstances is aware that Cambridge never had a chance from the beginning; and, in like manner, the race heat of 1877 must be regarded as a dead heat, though it is certain Oxford would have won but for an accident.

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Add to this the consideration that after Oxford had won nine successive races, from 1861 to 1869 inclusive, one of the best oarsmen Oxford has ever produced, Mr. George Morrison, gave much time and care to coaching the Cambridge crew into a better style than they had before followed, so that there is good reason for believing that in some of the races which followed (from 1870 to 1873, perhaps) the influence of Oxford training was at work in the Cambridge crews.

These points considered suggest a strong probability that there has been a radical difference for many years between the Cambridge and the Oxford style, the latter being the better. As it is well known that for many years since the old-fashioned racing-boats went out of use, the old-fashioned principles of rowing have been in vogue at Cambridge, we might fairly assume, apart from all dynamical evidence, that the old-fashioned stroke does not suit racing-boats of the present fashion.

Let us see what theory suggests as likely to be the best kind of stroke (for racing purposes) in these light boats, and then let us inquire what evidence we have to show that such a stroke really is rowed by the most successful crews.

(To be continued.)

FUTURE OF THE EARTH AND MOON.

By Dr. Ball, Astronomer-Royal for Ireland.

PART III.

EVERYONE knows that the moon always turns the same face towards the earth; this has been shown to be a consequence of the tides which were anciently raised in the moon. The tides in the moon were produced by the attraction of the earth, just as the tides on the earth are produced by the attraction of the moon. There is, however, an important difference; the earth is so much heavier than the moon, that the tides which the earth raised on the moon must have been much greater than the tides which the moon can raise on the earth. It matters not that the moon now contains no liquid ocean. All that is necessary is that the moon shall once have been soft enough to admit of being distorted by tidal influence. It must be remembered that it is not the mere presence of a high tide or a low tide that does the work. It is the rising and falling of the tide which produces the currents, and it is the tidal currents which do the work. The mighty tides which once acted on the moon have long since ceased, but they have forced the moon always to turn the same face to the earth, as this is the only attitude in which tides do no work on the moon. In the distant future the small mass of the moon will achieve the same result on the earth. At the final stage the earth and moon will move as if they were fixed rigidly together by movable bars, and were revolving around their common centre of gravity in 1,400 hours.

If the earth and the moon could be isolated from all external interference, there is no reason why this state of things should not continue indefinitely, but there is another disturbing cause with which we must reckon. We have seen that it was probably the sun which originally broke off the moon as a fragment from the earth. It seems, also, that the sun is destined to derange the harmonious compact in which the earth and the moon would have otherwise agreed. Once the 1,400 hour day and the 1,400 hour month have been reached, the earth will no longer be affected by tides produced by the moon. No doubt there will be a high tide on the earth and there will be a low tide, but as the earth will then always regard the moon with the same aspect, these tides will not rise or fall, they will not ebb and flow. There can then be no lunar tidal currents, and the tides will always remain at the same height at each point on our coasts. The sun, however, will still continue to produce tides on the earth. These tides will no doubt be small, as the solar tides are small at present; they will also rise and fall with extreme slowness. At present one high tide follows another in a little over six hours. At the final stage one solar high tide will follow another solar high tide only after an interval of about five weeks. These tides are small, and the currents they produce are very weak, but by incessant perseverance even these small tides cannot fail of producing an appreciable effect. The solar tidal currents act always in one direction, they always tend to retard the earth and to make the earth revolve more slowly. Here, then, we are conducted to a very remarkable condition of things in the distant future. The month will remain at 1,400 hours, while the day is lengthened still more. We thus have for the first time in the history of our earth-moon system the day actually longer than the month. A few years ago we knew of no analogy in the solar system to the state of things here foreshadowed. But the splendid discovery of the satellites of Mars has enabled us to give an illustration. The interior satellite of Mars moves round the planet in about seven hours, while the planet itself takes more than three times as long. At the time of its discovery this seemed a most anomalous circumstance, but now the difficulty has been to a great extent removed. It seems likely that Mars himself once rotated more rapidly than at present, and that by the intervention of the solar tides the present state of things has been brought about. It must not be overlooked that Mars and his satellites are much smaller than the earth and the moon. We might therefore expect to find that the process of evolution has proceeded much further in the case of Mars than in the case of the earth. Once the solar tides have acted on the earth sufficiently to abate its velocity below that of the moon, a new reaction on the moon will be manifested. This new influence is not a little curious. As the earth ceases to turn the same face to the moon, the lunar action will again commence to develope tides on the earth. The tidal currents produced in this way will tend to drag the earth on faster, instead of to retard it as before, but the moon can exert no action on the earth without a corresponding reaction. In this case the reaction will take the form of a force tending to draw the moon in again towards the earth. The matter is, however, too complicated for us to pursue it any further with advantage.

It is, indeed, remarkable that so striking a period in the earth-moon history can be traced out merely as limiting the influence of the tide. It would seem, from one aspect of the question, that at the present moment we are near the centre of the period, inasmuch as the ratio of the month to the day has but recently passed its maximum. From another point of view, however, we seem to be vastly nearer to the first stage, ancient as that is, than we are to the last. The day has lengthened from 3 hours to 24 hours, but the lengthening has to go on until the day lasts 1,400 hours, and the rate at which the change proceeds is now extremely slow, and is getting still slower. We cannot estimate the countless myriads of years that must elapse before the moon has attained its greatest distance and the day has become 1,400 hours in length.

We have in this paper merely touched on one department of the great problem of tidal evolution. The subject is yet in its infancy, but it seems to have before it a most noble future. By a series of most splendid discoveries, Lagrange had shown that the solar system contained the elements of stability and of permanence. Lagrange showed

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that the perturbation of the planets could never transcend certain narrow limits. The planes of the orbits could never be much deranged, the eccentricities of the orbits could never be much altered. The major axes of those orbits could hardly be changed at all. But all the great calculations of Lagrange involved one supposition, they all took for granted that the earth and moon, the sun and the planets, were rigid bodies. This we know is certainly not the case with many of these bodies, it is probably not the case with any one of them. Viewed with regard to the present researches, we are tempted to ask whether the absence of rigidity may affect the truth of Lagrange's great theorems. On the answer to this question depends the stability of the solar system. There can be no doubt that owing principally to their small sizes, and to the distances at which they are separated, the planets behave nearly as rigid bodies would so far as their mutual attractions are concerned. Lagrange's theorems are therefore approximately true, and they will remain substantially correct for hundreds of thousands of years, but for all that the planets are not rigid. Jupiter and Saturn do not act upon each other merely as the two heavy particles which Lagrange's theory would require. Jupiter and Saturn do, no doubt, attract each other as Lagrange supposed, but they also raise tides in each other. These tides may be despised when we are considering moderate periods of time, but the tides are there nevertheless. It may be slowly, but it is certainly surely, that these tides are doing their work, and by their agency the solar system in course of time shall become utterly transformed. Geometers know that when a curve has to be drawn, the various parts of that curve may be represented with all desirable accuracy by suitably chosen arcs of circles. As we proceed from one part of the curve to another, the radii and the centres of those circles are changed so that each arc of the circle shall be substantially coincident with the corresponding part of the curve. As the arcs of the circles coincide with the curve, so do the laws of Lagrange adapt themselves to the course of change in the universe. At any moment Lagrange's laws are practically true, but in immense periods of time the system undergoes profound modification. The laws of gravitation as ordinarily considered account for many of the *details* in the present architecture of the heavens For the profounder and grander truths of the universe it seems not improbable that we must interrogate the tides.

Errata.—In Part I. of "Photography for Amateurs" (No. 19, p. 400), paragraph 4, for "improved" read "impressed." In par 5, for "photoganic" read "photogenic"; and in the last par, read "J. Nicéphore, Niepce"; and for "figures" read "pictures." In No. 17, p. 363, Letter 289, for ". . . this gas; besides which, coal-gas is harmless," read ". . . this gas, beside which coal-gas is harmless."

Rights of Animals.—Of the grotesque ideas which have imposed on us in the solemn phraseology of divines and moralists, none is more absurd than the doctrine that our moral obligations stop short where the object of them does not happen to know them; and assures us that, because the brutes cannot call us to account for our transgressions, nothing that we can do will constitute a transgression. To absolve us from paying for a pair of boots because our bootmaker's ledger had unluckily been burned, would be altogether a parallel lesson in humanity. It is plain enough, indeed, that the creature who is (as we assume) without a conscience or moral arbitrament, must always be exonerated from guilt, no matter what it may do of hurt or evil; and the judicial proceedings against, and executions of, oxen and pigs in the Middle Ages for manslaughter were unspeakably absurd. But not less absurd, on the other side, is it to exonerate men, who *have* consciences and free will, when they are guilty of cruelty to brutes, on the plea—not that *they*—but the brutes, are immoral and irresponsible.—Frances Power Cobbe, in the *Cornhill Magazine*.

ILLUSIONS OF MOTION AND STROBIC CIRCLES.

By Thomas Foster.

IT is a long time since I last wrote about illusions. But other subjects of more immediate interest have filled all the available space.

The last illusion which I considered was a very singular one, and I have been rather surprised to notice how few among the readers of Knowledge have recognised its interest and significance. It is one which admits of various modifications, by which the real nature of the illusion may be put to the test. It will be remembered that in describing it, I noted that the paper held in the reader's hand—Knowledge—might be used to illustrate it. Rolling Knowledge into a tube an inch or so in diameter, and looking through this with one eye, while the hand not holding the tube is held touching the tube, at about the distance for distinct vision, an appearance is presented as though one were looking through the hand. There seems to be a distinctly outlined aperture through the palm of the hand, if the hand is held with the palm open, or through the fist (and this has an even stranger appearance) if the hand be clenched. Now, if the experiment be modified by using tinted, or strongly patterned paper for the tube, and by looking at variously coloured surfaces, it will be found that the strength of the illusion varies notably. (This may be tested by directing the mind's attention specially to the hand, so as gradually to overcome the absurd delusion by which one seems to look through the palm or the fist.) I leave this as an exercise to the student of illusions, noting that the result to which my own experiments have led me is this, that in ordinary vision what the left eye sees is referred by the mind (unconsciously, of course) to the right eye, and what the right eye sees is referred to the left eye. By a series of tests, such as those I have indicated, this peculiarity may be recognised. I believe the eyes might be trained to overcome illusions of this class.

And now let us turn to some illusions of apparent motion, which, like those we have already considered, can be explained if carefully studied.

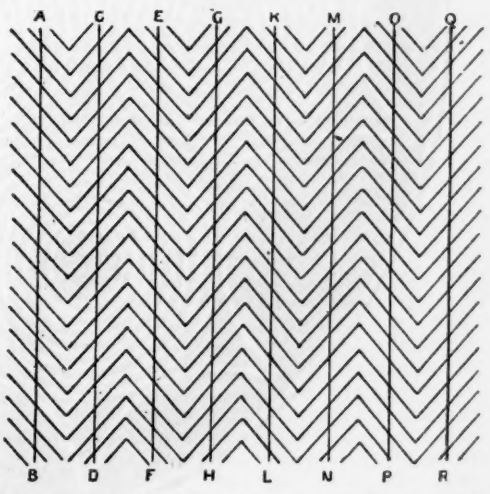


Fig. 1.—Illustrating Illusions of Direction, Shading, and Motion.

It may be remembered that in considering Fig. 1, in the first number of Knowledge, I drew attention to the fact that the lines AB, CD, EF, &c., which are in reality

Fig. 2.

Fig. 4.

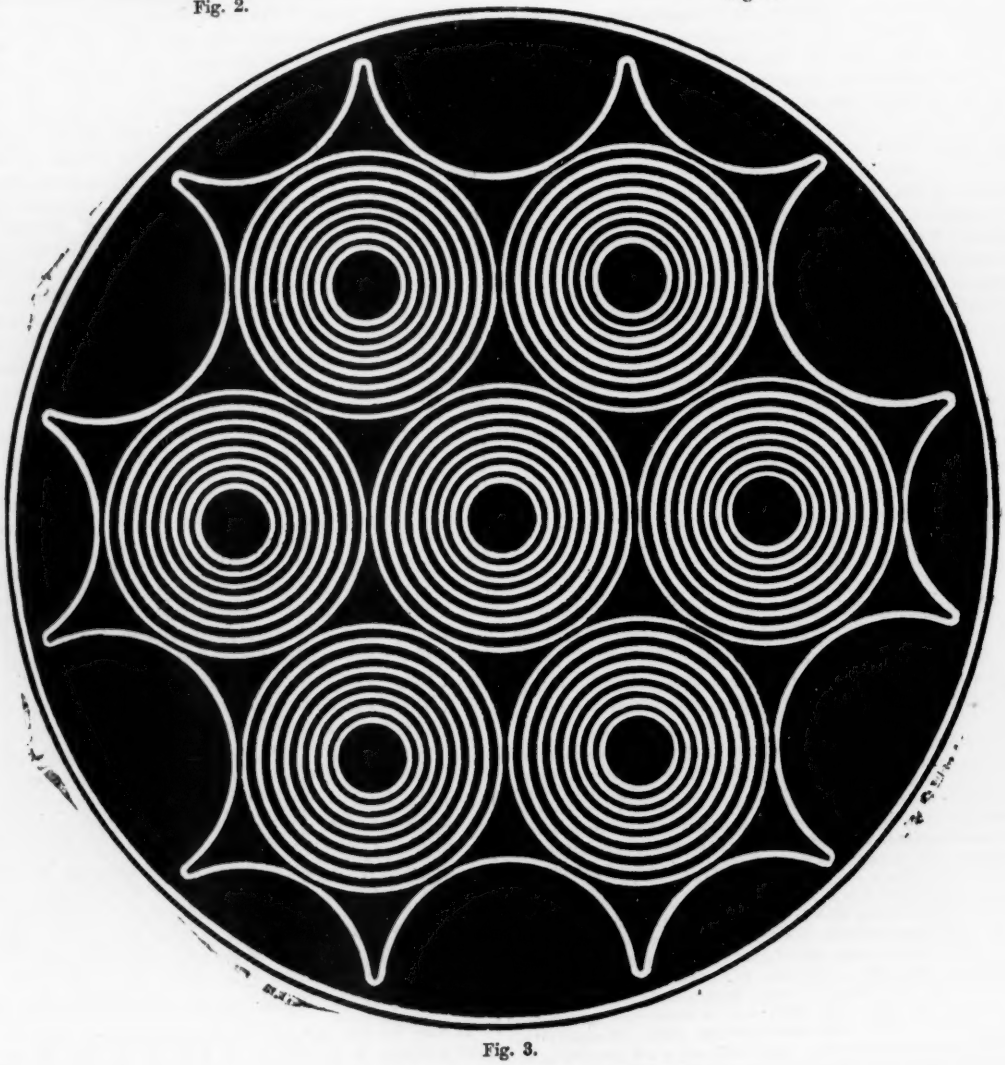


Fig. 3.

ILLUSTRATIONS OF STROBIC CIRCLES.

parallel, appear to converge towards AC, BD, CE, &c. Later (in No. 4), I called attention to the fact that this figure illustrates an illusion of motion. If the eye be run up and down the parallels, these appear to move. When the eye is at rest, they seem alternately convergent, especially if the figure is viewed a little askew, being neither held with the parallels vertical nor horizontal. But if the eye runs along two parallels which before had seemed to converge, they are found not to converge, and the effect produced is as though they had moved from each other at the end towards which they had seemed to converge. Another effect, also, is noticed. The level surface on which are the zigzag sets of lines, appears to be ridged, this being apparently due to the fact that the alternate sets of close parallels are viewed in different aspects. Thus, suppose the paper held so that B, Fig. 1, is lowest, then the parallels in sections CD, GH, MN, OR, appear farther apart than those in the other, or alternate sections. Now, if the figure is rotated in its own plane, so that first HL, then R comes lowest, there is a change from the appearance just described to its reverse, the parallels in sections CD, GH, MN, OR, appearing now closer instead of farther apart than the others. Accompanying this change will be found certainly singular and rather complicated appearances of motion, the ridges sinking, then rising again, the sets of close parallels drawing apart or closer (and, if the motion is continued, closing up again, and drawing apart again respectively), and the other sets of parallels (vertical in the picture) seeming to bend and grow straight alternately.

Leaving the reader to study these changes and to note that they correspond with what we should anticipate, we proceed to more familiar instances of apparent motion, which find their explanation, I believe, in what we have learned from Fig. 1.*

Consider Figs. 2 and 3, first at rest, and then as each appears when a slow circling movement is given to it (as when a saucer is so moved as to set a small quantity of liquid in it circling round near the edge), and then as each appears when swayed through a short distance from side to side, or from and towards you, in its own plane.

When looking at either picture, held perfectly at rest, the eye, if kept still, is presently affected by appearances of quivering motion, apparently affecting the entire picture. If the eye moves backwards and forwards across the picture, or irregularly over it, the sets of concentric circles appear to undergo partial rotations,—in alternate directions in one case, irregular in the other. When the pictures are made to circle in their own plane, all the sets of concentric circles seem to turn round in the direction of the circling motion given to the picture. Lastly, when the pictures are shifted back and forward in their own plane, from side to side, each set of concentric circles is blurred at the sides, or rather in the side quadrants, distinct in the upper and lower quadrants, regarding the sets as divided into quadrants by diameters situated thus ×. When the motion is from and towards the observer, the upper and lower quadrants of the sets of circles are blurred, the side quadrants distinct.

To produce the rotatory motion, it is necessary to give a tolerably rapid circling motion to either picture. The experiment succeeds better if the picture is mounted on card, and the circling motion is communicated by a suitable crank, so as to be more uniform than any motion which can be given with the hand.

When Fig. 4 is swayed like the others, by a circling motion in its own plane, the small black discs seem to be carried round in a direction contrary to that of the circling. It will be noticed that if the circle in which this figure is swayed is somewhat larger than is necessary to produce the deceptive appearance of motion, a regular pattern seems to be formed, by the persistence of the visual images of the small black discs in the picture.

* At the meeting of the British Association in 1877 (at Plymouth), Professor Sylvanus Thompson exhibited these singular illusions.

NEWCOMB'S POPULAR ASTRONOMY.

WE turn now to the less pleasant task of pointing out defects which might mislead those who rely on Professor Newcomb's well-deserved eminence as a mathematician and an astronomer.

We should have been disappointed if such subjects as the tides, the precession of the equinoxes, &c., had been simply left unexplained in a work of this character. Few subjects are less satisfactorily explained in most works on astronomy than the tides, for instance. We are supplied over and over again with the statement that the water immediately under the moon is drawn from the earth, while the earth is drawn from the water at the opposite side, a statement true enough (when properly limited) in itself, and a necessary preliminary to any explanation of the tides. But there the usual explanation comes to an end, the student being simply told that but for friction there would be high water in the region under the moon and at the antipodes of that region. Now, what would be thought of an explanation of the motion of a reeling top which only showed that an inclined top tends to tumble over? The reader would assuredly say, "What I want to know is why the top when spinning does *not* tumble over, but reels round." The common explanation of the tides is open to precisely such an objection. In the actual case of the rotating earth there would be low water instead of high in the regions under the moon, and opposite, were there no friction; and the effect of friction is not to throw back the place of high water about half a quadrant, but about three half quadrants. These are the relations which have to be explained; whereas the ordinary explanation deals with relations which have no existence, not even a theoretical existence, in nature. Now, it would have been very useful if Professor Newcomb had given an original and effective explanation of the true theory of the tides. Sir G. Airy has given one, but it is not in the books; Sir Edmund Beckett has given another, in his fine work, "Astronomy without Mathematics"; but the conditions he imposed on himself prevented him from giving the best explanation, though he has avoided mathematical reasoning. We should, then, have been disappointed if we had merely found that Professor Newcomb had left the tides entirely unexplained, or if he had simply repeated the usual incomplete explanation. Unfortunately, he has done worse than this. He has given an explanation which is entirely incorrect. He describes the earth, and truly, as circling once in a month around the common centre of gravity of the earth and moon; but he attributes to different parts of the earth as she thus moves different degrees of centrifugal force, according to the distance of each part from the centre of gravity just mentioned. If these different degrees of centrifugal force (or rather tendency) in reality existed, the tides would be far more important phenomena than they really are. A calculation which Professor Newcomb might have made on his thumb-nail as he wrote the

passage would have shown that the tidal action of the moon alone would be thirty times greater than the real action of the sun and moon together. In reality, the difference of centrifugal tendencies thus imagined has no existence—a circumstance which no one would have recognised more readily than Professor Newcomb, had he thought of examining the matter carefully. The motion of the earth around the common centre of gravity of the earth and moon may be thus illustrated:—Conceive a ball 8 in. in diameter suspended by a long thread, and not rotating; then imagine the point of suspension carried steadily round in a horizontal circle 6 in. in diameter. Thus the centre of the ball will be carried round, and so will every point in the ball, in a horizontal circle of the same diameter. Neglecting the earth's rotation on her axis, which is an independent movement, her motion round the centre of gravity of her own mass and the moon's takes place in this manner, each point in the earth (whose diameter is about 7,900 miles) describing, once in a lunar month, a nearly circular orbit about 6,000 miles in diameter. Professor Newcomb's mistake consists in supposing that the motion is of a very different kind, such, for instance, as our illustrative ball would possess, if it were twirled round on a knitting-needle thrust through it at a distance of 3in. from the centre, and held in an unchanged upright position while rotated, carrying the ball with it. In this case, points near the needle would move in small circles, while points farther away would travel in large circles, those furthest off travelling in a circle 14in. in diameter. Thus there would be different centrifugal tendencies in the different parts of the ball. If the earth moved in this way round the centre of her monthly orbit, while also rotating on her axis once a day, and round the sun once a year, we should have the state of things imagined by Professor Newcomb. But our opportunities for observing the result would be precarious; for the tidal waves would be of portentous magnitude, and at least half the present land surface of the earth would be uninhabitable. It is strange that it should not have occurred to Professor Newcomb that, if his explanation of the lunar tides were correct, the solar tides explained on the same principle would be utterly insignificant compared with the lunar ones, instead of bearing to these about the proportion of 2 to 5. His mistake in this matter is a curious illustration of the errors into which even the profoundest mathematicians may fall in careless moods. It can only be compared with one which Lord Brougham is said to have made in one of the earliest publications of the Society for Diffusing Useful Knowledge. Professor Tait, in a lecture delivered before the British Association at Glasgow, stated that in such a treatise, quickly withdrawn from publication, Brougham explained that a man carries a load more readily over his shoulder than suspended from his hand, because in the former case it is further from the centre of the earth, and gravity diminishes as the square of the distance from the earth's centre increases. The story seems incredible, but it is scarcely more remarkable than that a mathematician like Newcomb should employ reasoning as unsound in reality as that of those who deny the moon's rotation. In fact, Newcomb's paradox is very similar in character to that of Messrs. Jellinger Symons and H. Perigal, though not quite so obviously erroneous.

It is in some respects even more remarkable that Professor Newcomb should have given an equally erroneous explanation of the precession of the exquinoxes, or rather of the motion to which precession is due—the reeling of the earth like a mighty top, each reel lasting for the long period of 25,890 years. The subject is, indeed, far more difficult to explain to the non-mathematical student than the tides. But for that very reason we should have expected to find our author on his guard against mistakes. The ablest mathematician may trip in explaining off-hand an easy subject, precisely as the ablest gymnast may fail when lightly essaying some simple feat. But in dealing with such a subject as the precession of the equinoxes, even the ablest mathematician girds up his loins as for a task of difficulty. Yet Newcomb's explanation of the phenomenon is altogether erroneous, though his statements respecting the nature of the phenomena are, of course, entirely correct.*

The explanation of the peculiarities which theory indicates as affecting the figure of the moon, though observation has not yet demonstrated their actual existence, is also erroneous. It brings our author so close to the paradox of Jellinger Symons (earlier propounded by Bentley), that one cannot but wonder how he failed to notice the mistake which underlies his reasoning.

The account of the Harton Colliery experiment for determining the mass of the earth is incorrect, and the principles on which the experiment depends are not properly stated. Professor Newcomb says that if the density of the earth increases as we approach the centre, the diminution of the force of gravity will be less rapid as we descend. But in reality the actual increase of density towards the earth's centre causes gravity to increase as the depth below the surface increases. This increase continues to a depth hundreds of times greater than can be reached by man. Our author goes on to say that "a determination of the density of the earth by the diminution of gravity in a mine was made by Professor Airy at the Harton Colliery in 1855." But in this experiment Airy found gravity greater at the bottom of the mine than at the top. Owing to this increase in the force of gravity, the pendulum at a depth of 1,260 ft. gained $2\frac{1}{4}$ seconds per day, as compared with its indications at the mouth of the mine. It is, by the way, worth noticing, though so far as we know the point is not mentioned in any of our treatises on astronomy, that even if the density of the earth were uniform, gravity at the bottom of a deep opening would be greater or less than at the surface, according to the shape of the opening. It can be shown that if the earth were at uniform density, the action of gravity on a body at the bottom of an opening would be equal to the attraction which a mass equal in all respects to the removed matter would exert on that body (only this attraction must be regarded as acting towards the earth's centre), added to the attraction due to the body's distance from the centre. The latter portion is less than gravity at the surface, in just the same degree that the distance of the body from the earth's centre is less than the earth's radius; but the

* Some one in America, criticising the astronomical articles in the "American Cyclopædia" (Appleton), which were revised, and in great part re-written, by me, was kind enough to point out that my explanation of the precession of the equinoxes was not new. It was not mine, but was left by me almost untouched, being well written, and correct. I left, for a like reason, the matter relating to precession in the "Encyclopædia Britannica" almost untouched when the article on astronomy was entrusted to me for revision and re-writing. Possibly, if I had endeavoured to find an entirely new explanation, I might, like Professor Newcomb, have come upon one which, though new, was not true. At any rate, where so skilful a mathematician went astray, none need be ashamed to err. I believe that in a later edition of Professor Newcomb's book, the errors pointed out above, and in my earlier review in the *Contemporary*, have been corrected. They were, at any rate, corrected in American journals. Professor Newcomb is one of those who are strong enough to be able and willing frankly to admit and correct such mistakes as all active thinkers are bound to make from time to time.—Ed.]

other portion may more than make up for the deficiency, if the opening is wide enough. The mine method of determining the earth's mean density is, in any case, subject to great uncertainty; and few astronomers now attach much weight to the result of the Harton Colliery experiment.

There is a singular mistake in the following statement respecting Venus in transit:—"It would not be possible to see any indications of an atmosphere in such circumstances; for the reason that the light passing through its denser portions would be refracted entirely out of its course, so as not to reach an observer on the earth at all." By similar reasoning, it could be shown that we ought never to see the sun. For the rays which set out from him directly towards an observer on earth are refracted entirely out of their course, and never reach the observer at all. Precisely, however, as such an observer sees the sun by rays which but for our atmosphere could never have reached him, so solar rays pass through the atmosphere of Venus in transit to an observer on earth, which would never have reached him but for that atmosphere. In fact, the solar light seen close to the black disc of Venus in transit does not come from the part of the sun immediately beyond Venus, but from other parts, or, to speak more correctly, from every other part of the sun's disc.

Professor Newcomb disposes rather too summarily of the interesting discovery made by Kirkwood, that there are gaps in the mean distances of the small planets from the sun, none travelling at $\frac{1}{2}$, $\frac{1}{3}$, 2-5ths, 3-7ths, &c., of the mean distance of Jupiter. "Whether these gaps are really due to the relations of the periodic times, or are simply the result of chance, cannot yet be settled." He says, "the fact that quite a number of the small planets have a period very nearly three-eighths that of Jupiter may lead us to wait for further evidence before concluding that we have to deal with a real law of nature in the cases pointed out by Professor Kirkwood." The law involved is most important in its consequences, leading almost inevitably to the rejection of the simple nebular hypothesis of the solar system, and to the adoption of a theory of the development of the system in large part by meteoric accretion. We may be excused, therefore, for dwelling upon a point which, at a first view, may seem to belong rather to the smaller details of astronomical research than to the broad facts in which the general public take interest. If Professor Newcomb had constructed a graphical delineation of the distances of the smaller planets, he would have rejected at once, we believe, the idea that chance has anything to do with the relation in question. Such a delineation we have before us as we write, and we find the smaller planets most markedly divided into five principal families, the spaces separating which correspond to the following periods, or sets of periods (Jupiter's period being taken as 1):—(i.) $\frac{1}{4}$, 2-7ths, and 3-10ths; (ii.) $\frac{1}{3}$; (iii.) 2-5ths; (iv.) 3-7ths; (v.) $\frac{1}{2}$; and (vi.) 3-5ths. Now the distance corresponding to the period $\frac{3}{8}$ falls in the very heart of the richest of all these sub-families. Here we could hardly expect to find a gap, especially when we remember that the perturbing action due to the correspondence between three periods of Jupiter and eight periods of a small planet would be very small compared with the disturbance due to simpler relations—as where four, three, or two periods of a minor planet correspond with one period of Jupiter, or five periods of a minor planet with two or with three periods of Jupiter. But in reality, the richest sub-family of small planets does open out unmistakably at the distance corresponding to a period equal to 3-8ths of Jupiter's. This distance would be 2·7055, the earth's being unity. Now, there are six of the small planets whose distances lie between 2·686 and 2·701, and six whose distances lie between 2·708 and 2·721; but there is not one whose distance lies between 2·701 and 2·708. In other words, whereas the average interval between successive distances amounts only to 0·0025 in the groups on either side of the critical distance we are considering, there is a gap of 0·007, or nearly three times as great, betweeh these groups.

We have left to ourselves no space to comment on the more speculative opinions expressed by Professor Newcomb. On the whole, he seems to us to show at once more caution and more daring than most of his contemporaries—more caution, inasmuch as he does not accept old-fashioned views without carefully inquiring into them; more daring, inasmuch as he is not afraid to accept new views when he finds that there is strong evidence in their favour. Here and there, as where he ascribes the darkness of certain zones of Saturn's rings rather to the blackness of their component satellites than to sparseness of distribution, we are unable to agree with him. But it is refreshing to find an official astronomer, and especially one in Newcomb's high position, prepared to analyse and weigh evidence, instead of merely recording what has been observed. In fine, though we have thought it our duty to point out some errors which might mislead the general reader, we can cordially recommend Professor Newcomb's "Popular Astronomy" as the finest general treatise on the subject since Sir J. Herschel published his celebrated "Outlines of Astronomy."

THE CRYSTAL PALACE ELECTRICAL EXHIBITION.

SIXTH NOTICE.

WE commence this week a description of the various systems of incandescent lighting now being exhibited at the Crystal Palace. Before describing the different lamps, a word or two on their general principles may not be out of place. In the first instance, we must understand that a current of electricity, in passing through a substance, tends to heat it in exact proportion to the difficulty experienced by the current in so passing, just as a flow of water in traversing a pipe brings about a greater or less exaltation of temperature. This arises from a never-failing law that, where motive power or force is hindered or opposed, that force is converted into another force which we know as heat. Theoretically, no substance allows electricity to pass through it unimpeded, any more than water can pass through a pipe without having more or less friction to overcome. In practice, we allow a pipe large enough to carry the water, without having to resort to any undue pressure; so also in sending an electric current from place to place, we provide a good *conductor* of electricity for the purpose. Furthermore, suppose, in the case of the water-flow, that the pipe is not uniformly wide or smooth, or that here and there quantities of sand, &c., have accumulated, then at these particular places the flow will be impeded, and heat produced, while the general rate of progress can only be maintained by applying *extra* force to push the water forward. Could we measure the heat produced, we should find that it just equals the extra force necessitated by the opposition. This has also its analogy in electricity. Let our large smooth pipe be represented by the good conductor of comparatively thick copper wire, and let the obstacle in the pipe have its counterpart in the electrical circuit by inserting a very thin piece of conducting sub-

XUM

stance, such as fine platinum wire, then, on passing the current, heat is produced in the thin conductor, because of the opposition or *resistance* its thinness presents to the current. This heat accumulates, so to speak, with the current, and, ultimately, the thin conductor emits a pure white light. Our readers must please understand that light is but the manifestation of a considerable exaltation of temperature.

We have mentioned platinum as the thin conductor; the reason for this is that all metals, &c., even when of the same size, do not conduct with the same readiness or facility, copper being about the best, and platinum the poorest of metals. It then follows that a thin platinum wire (resembling the small pipe with sand in it) will offer more resistance than a copper wire of the same gauge, and, accordingly, will get hotter, and so give out more light. Even, however, if the copper were equal in its resistance, its physical features would, in this instance, preclude its use. It would, in fact, melt, or even volatilise.

Such was the first form of incandescent lighting. It did not, however, prove very satisfactory. Bodies offering more resistance than platinum were required, and it is only natural that carbon in some form or another should be resorted to. It was found, on trying it, that union was made with the oxygen of the air, converting the filament into carbonic anhydride gas. It occurred to Mr. Swan about twenty years since that, if a *perfect* vacuum could be obtained, the carbon would be made more durable, as there would be no oxygen for it to combine with. It is on this ground that Mr. Swan claims, and is almost universally considered to be, the prime inventor of the incandescent lamp. The necessary vacuum, however, could not then be obtained, but more recent inventions in pneumatics have enabled electricians to achieve their

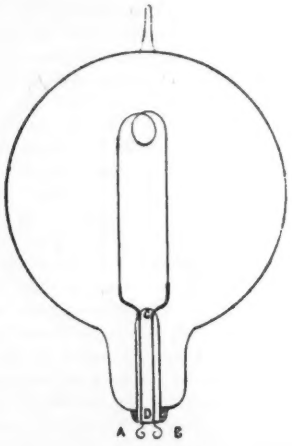


Fig. 1.

object. Even with this vacuum, it was found that the filaments were not so durable as had been hoped, and Mr. Swan must again be credited with discovering the reason. He demonstrated the cause to be the presence of particles of air, &c., located in the spaces separating the particles of the filaments, and by raising his filament to a very high temperature he expelled the air. In consequence, we secure, to all intents and purposes, a perfect vacuum. There being nothing with which the carbon may unite, the supposition forced upon us is that the filament should last for ever. Such an idea, however, is erroneous. Doubtless our readers are aware that in an arc lamp the two carbon rods are consumed, one of them twice as rapidly as the other. This phenomenon is to some extent due to the current, in passing, tearing away the particles of carbon from one rod, and depositing them on the other. This also occurs in incandescent lamps, although to a modified and very much smaller extent. The current, in passing through the filament, tears away minute particles on entering and deposits them on leaving, so that one end is gradually diminished and eventually fractured. This, however, is a process of time ranging from 600 to 1,000 hours continuous burning.

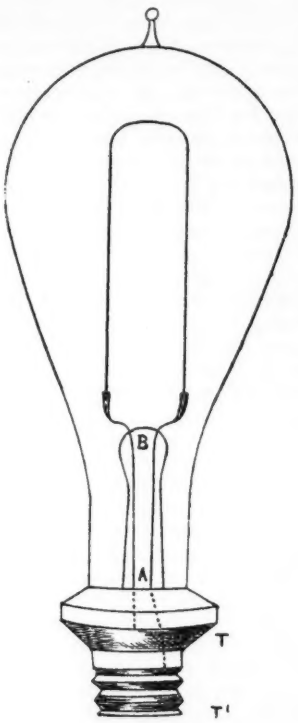


Fig. 2.

The different forms of lamp exhibited at Sydenham are the Swan, Edison, Lane-Fox, Maxim, and British.

The Swan (Fig. 1) is the invention of Mr. Swan, of Newcastle-on-Tyne. It consists of a glass globe nearly 2 in. in diameter. A glass rod, C D, with the two platinum wires, A and B, passing through it, is fused into the globe. The inside ends of the wires are attached to the ends of the carbon filament, which is made from cardboard (Bristol board), and the free or external ends are connected to wires from the machine generating the current.

Fig. 2 represents an "Edison" lamp. It consists of a glass bulb, about 5 in. long. The filament is made of bamboo fibre, one end being, by a very ingenious device, made slightly thicker than the other. Platinum wires are attached to the filament, the junctions being coated, electrically, with a little copper. The platinum wires are fused into the glass tube AB, at B, and are shown in the diagram to be continued, through a kind of cap of plaster of Paris, to a brass band, T, and brass screw-thread, T', respectively. The lamp is then screwed into a corresponding socket, so that T' is in contact with another screw-thread, having attached to it a wire from the machine. The other machine wire is connected to a brass band in the socket which comes into contact with T, and so

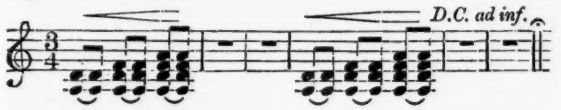
completes the circuit. The subject will be continued next week.

P.S.—The engraver made a little mistake in last week's Fig. 2. The fourth needle (from the left) should have been deflected to the left, so as to point to the letter "B."

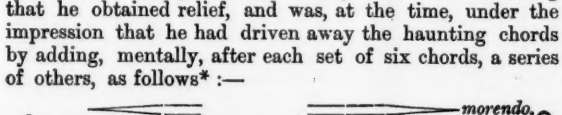
BRAIN TROUBLES.

THE ECHO SIGN.

A SYMPTOM called the "*echo*" sign, which usually indicates very serious brain mischief, has, like other such signs, its analogue among the symptoms of minor mental trouble. Most of us have noticed how, when we are weary and overworked, we are apt to repeat mentally words or sounds which we have heard or had occasion to utter. Sometimes the tendency becomes exceedingly annoying—a circumstance which, though not necessarily indicating serious mischief, must be regarded as a warning not to be neglected. On one occasion, a time of great domestic trouble, the writer was haunted for two or three days in succession by these three chords, repeated in the way



here indicated. They had been first heard (or imagined perhaps) during church-time on a calm still Sunday, when, after several days of cold and bitter winter weather, the sun shone brightly, and the air was warm and pleasant. There was illness "even unto death" in the house, and a loss such as changes the colour of life was approaching. But, probably, long and anxious night-watching had more to do with this strange affection of the mind than fear or sorrow. The haunting chords ceased only during sleep (a trained nurse had the evening before taken the writer's place); when consciousness returned after heavy, dreamless sleep, the chords were heard again, now loud and clear, anon distant and indistinct, usually in slow succession, with rather long intervals between each triplet, but at times less slowly and with scarcely the intermission of a single bar's rest. At another time, the writer would probably have been rendered exceedingly anxious by the monotonous repetition of these mental sounds, though he might have found it difficult to determine whether they indicated or were the cause of mental mischief. As it was, other thoughts engrossed his mind too much to allow of any anxiety on this account; and after a few days the chords ceased to trouble him; though to this day he is careful not to allow the mental voice to utter these sounds to the mental ear, lest again the chords should begin to be monotonously repeated. It is probable that this particular mental trouble ceased as it began, apart from any act on the writer's own part; still it may be worth mentioning that he obtained relief, and was, at the time, under the impression that he had driven away the haunting chords by adding, mentally, after each set of six chords, a series of others, as follows* :—



* It is probably not necessary for the writer to explain to musicians that he knows nothing whatever of harmony. Perhaps the above arrangements of chords is full of mistakes, so far as the laws of harmony are concerned, but it represents exactly, first, the chords which troubled the writer, and secondly, those which he added to put the former out of his head.

He noticed that the interval before the paired chords began to be mentally heard again, gradually increased, after the above plan had been followed, until the intervals of silence became so long that the mind could, as it were, forget that it was troubled by these haunting notes.

The "echo" or repetition sign, as we have said, is commonly indicative of serious cerebral mischief. Dr. Winslow was of opinion that it arose, to some extent, from that sluggish and abstracted state of thought, amounting to reverie, which is so often seen in cases of long-existing and sometimes undetected affections of the brain. "The mind seems incapable," he says, "of apprehending, under these circumstances, the most simple questions, and, parrot-like, repeats them. I have noticed this symptom in other conditions of depressed vital and nervous power, but it more particularly accompanies softening of some portion of the brain." It can scarcely be doubted that the monotonous mental repetition of words or sounds is indicative of mental trouble; yet not necessarily or probably of any really serious mischief. Rest or change of occupation will in general prove a sufficient remedy. If not, it is time to seek for advice, though rather from a sensible general practitioner (preferably a family doctor) than from those who have directed special attention to cerebral diseases; for the latter are apt to alarm patients by suggesting the possibility, or even the probability, of approaching mental derangement.

As an illustration at once of the morbid phenomena of speech, and of the tendency among certain students of mental disease to exaggerate the significance of such phenomena, we may take the following passage from Dr. Forbes Winslow's book:—"It will not be out of place," he says, "to direct attention to a precursory symptom, not only of approaching paralysis, but of insanity. I allude to the practice of many patients suffering from incipient brain and mind disease, of talking aloud when alone. A distinguished physician observed this symptom to precede an attack of paralysis, in the case of a nobleman who for many years was Prime Minister of this country. In many cases of irritation of the brain, as well as of structural disease, the patient is observed to talk to himself, and the commencement of insanity is often detected by this symptom" True, Dr. Winslow goes on to say that this eccentric habit is consistent with a perfect state of health of body and mind; but these few words suggesting comfort to those who occasionally talk to themselves, are likely to be overlooked in a long passage indicating this common habit as one of the signs of approaching insanity.

SCIENCE AND RELIGION.—The corruption of philosophy, by the mixing of it up with theology, is of wide extent, and is most injurious to it, both as a whole and in parts. . . . . This folly is the more to be prevented and restrained, because not only fantastical philosophy, but heretical religion spring from the absurd mixture of matters divine and human. It is wise, therefore, to render unto faith the things that are faith's.—Bacon's "Novum Organum."

EYESIGHT OF DOGS.—Kindly allow me to add my testimony to that of "G. S. S.," under above heading in your issue of March 3. Having been present at many sheep-dog trials in Merionethshire, Montgomeryshire, and Cardiganshire, I am able to assert that sheep-dogs at least have better sight than the average man. I have seen these remarkable dogs, notably at Machynlleth Park, when the signal has been given, run straight to where two sheep had been let loose about half a mile distant, up a hill, covered in places with gorse. Oftentimes the sheep have been out of sight to the spectators when they have been seen by the sheep-dogs, and brought to the bottom of the hill. My experience of dogs leads me to the belief that they are anything but near-sighted. When in the Isle of Wight, I had in my possession a large retriever bitch, that would see me coming along the Whippingham-road long before I was able to recognise the animal.—VERITAS.

XUM

EASY LESSONS IN BLOWPIPE CHEMISTRY.

By Lieut.-Colonel W. A. Ross, late R.A.

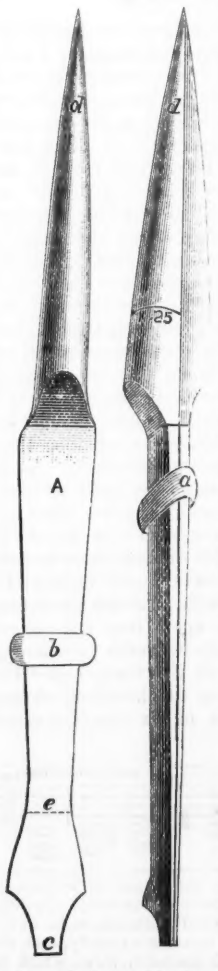
Lesson IV.—GOLD, SILVER, IRON, AND MANGANESE—SILICA—TIN—MAGNETICAL PLIERS.

THE purple auriferous, or gold-bearing bead, if now kept steadily for some time in HP, shows, through a lens, minute spangles of gold again, and these reactions can be repeated by a good operator over and over again. To sum up the gold reactions. We have P acid doing BB what no other known acid in chemistry is capable of doing—dissolving pure gold. We then have this dissolved gold caused by the blowpipe to exist in three states of oxidation in its bead: (*a*) as a transparent, colourless, solution, after a long PP (a pyroxide which, as we shall see afterwards, can also be obtained by treatment on aluminium plate); (*b*) as a purple solution, after a half-inch PP; and (*c*) as a brownish-"muddy" solution (blue by transmitted light), after OP. Yet we find it stated in standard works on chemistry and metallurgy, that gold is incapable of pyrological oxidation! Let us now try in our bead that other desirable metal, *silver*. If you scrape, with a penknife, the rim of a sixpence or shilling over a sheet of paper, you get a few specks of silver, more than sufficient for our purpose; and, perhaps, her Majesty's Mint will not consider it worth their while to prosecute me for recommending such an illegal process in the pursuit, or rather assistance, of Knowledge. These specks are taken up at bottom of the hot P. acid bead, precisely as the piece of gold leaf was; but a very different result is the consequence of OP. Instead of the "muddy" bead in the latter case, the little silver ball which immediately coheres at the bottom is rapidly oxidised, and gives out a quantity of yellow, opaque matter, like cream, into the bead, flecked here and there with red spots and dashes. In fact (to compare small things with great), our P. acid bead now looks, through a lens, something like a liliputian dish of strawberries and cream, with *very* few strawberries. I will tell you all about the "lens" presently. Our bead is now "argentiferous,"* and the yellow matter (which, I suppose, is pyrophosphate of silver) is rapidly dissolved in this powerful acid, first to an appearance very like that of a thin tortoise-shell comb held against a window; then (after a half-inch PP) to a transparent, colourless bead, which you would never suppose contained silver or anything else. It looks like a drop of pellucid water on the platinum-wire ring. Now, I am going to try the mettle of my student. The following reaction, discovered by me in India in 1869, is rather difficult, but only requires practice until success attends patience and perseverance. The colourless argentiferous bead is plunged, but only for a moment, *into* the "tip" of the blue pyrocone. When drawn out it is found to present a most beautiful and perfect imitation of a *pearl*. The silver-phosphate just under the surface of the bead has been reduced to the metallic state, and shining like silver through the coating or glazing of phosphoric acid, gives the bead the exact appearance of a pearl.

The next substance I propose to try BB in P. acid is that honest prop of our native manufactures—iron. I don't think that, on the whole, rust is a difficult or expensive thing to obtain in this country, especially when one can afford to keep housemaids! However that may be, rust is an oxide of iron, containing two parts of metal to three of oxygen, and is, therefore, thus symbolised by the chemist, $Fe_2 O_3$.† A considerable quantity of finely powdered rust is taken up on the hot P acid bead, and dissolved by OP; for it is not a highly colouring oxide like that of cobalt or manganese. The bead is very soon topaz-yellow hot, but dissolves a quantity of rust before it retains any colour when cool. Eventually it looks exactly like a drop of watery blood; and I thought that if I now dissolved in it by OP a slight trace of MnO_2, the bead would assume the bright crimson colour of arterial blood; after HP the dark colour of venous blood. Chemists have long ago proved that animal (warm) blood contains iron and phosphoric acid, and I tried this little experiment in order to see if the brilliant crimson colour evidently produced in it by lung-oxidation, might be due to the presence of a trace of manganese in the iron of the blood; I found, however, that the Mn and Fe destroyed each other's colour. I now come to try pyrologically, a mineral which is rather too common, especially in the London streets. I refer to mud; or, as it is more politely called, moistened clay, and still more refined by the chemist into the term "Aluminium-silicate,"‡ which I shall here shorten to Al. silicate.

The treatment of Al. silicate in P. Acid PP. affords one of the most important chemical reactions of the latter, because silica is (except the rare "earth" zirconia) the only substance which is thus insoluble in it. Consequently, when we thus treat Al. silicate (or any other silicate) in it, the alumina or other substance not silica, is at once dissolved, and the silica left as a crystalline mass, more or less thick in proportion to the quantity in which it exists in the mineral. The way I proceed is this:—I first dissolve a single speck of cobalt oxide in my P acid bead, which makes it pink. I then treat in it the finely-powdered mineral, which I suspect contains silica, and, perhaps, an alkali, as soda or potash. The silica remains undissolved; the alkali dissolving, turns the pink bead blue; alumina in considerable quantity dissolving in absence of alkali, turns the bead mauve colour. Two great Blowpipe-chemists or Pyrologists—Berzelius (a Swede) and Plattner (a German)—used the substance called "microcosmic salt," which is practically a phosphate of sodium, in this way, but the soda of this bead so rapidly dissolves silica as to make its non-solution scarcely any test at all. The whole thing depends upon the amount of heat applied; and I can dissolve to a colourless transparent bead almost any quantity of Al. silicate in Mic. salt, by simply using a powerful blast with my blowpipe.

Natural oxide of tin (*Cassiterite*—Greek, *Kassiteros*, tin) which generally contains a little iron and manganese, may be also usefully treated in P. acid, thus: tin oxide is with difficulty soluble in P. acid, and requires great heat; iron and manganese oxides, on the contrary, are extremely soluble in it by moderate heat, so that, by holding the bead having cassiterite powder taken upon it in a position where it is affected by a moderate (blowpipe) temperature only, as in a half-inch PP, you obtain the same effect as the chemist does when he uses a dilute acid—that is, you dissolve the Fe. and Mn. oxides, leaving oxide of tin, which can be dissolved out of the bead in nearly a pure state by boiling water, and filtering the solution of iron and manganese.



I shall now conclude this lesson with a description and drawing of an implement (the watch-maker's pliers) mentioned in Lesson II., which I have modified and altered so as to make it answer several pyrological purposes when required; and these uses are all very much, and often, required. I have got an ironsmith of Acton (Mr. Poore) to make me one, from which this sketch is taken; and very nicely he has made it. He has also promised to make such pliers, and other blowpipe implements, at a moderate price (I will give it in the next lesson) for poor pyrologists, if required in sufficient number; in fact, I suppose he calls himself a Poore-pyrologist already, for he has been taking measurements of my hot-gas burner, in order to make one for himself. The figure, I think, which is of the natural size, explains itself. The large round legs (apparently, perhaps, too large, but not so when practically tested) enable the operator, by holding the very tip of a straight platinum wire, held firmly between them from below at the part marked ·25, and gently turning the pliers round with his right hand towards his left, which holds the wire, to make a good round ring ·25 or quarter of an inch in diameter, such as that figured in Lesson II. The wire is straightened and cleaned (as explained in Lesson II.) by squeezing and drawing it gently through the broad, flat part of the legs at A, and this process is much more effectual than that generally recommended by chemists, to dip the red-hot wire in hydrochloric acid, which also most certainly rots the wire, and causes it to break off after a time.

The ringed wire is now fixed between the legs as far as *d* (not further) by drawing up the brass collar *a* to the position shown; by drawing it down to *b*, the legs of the pliers are left slightly open, enabling them to pick up small fragments; by drawing it to the very bottom *e*, the legs are left wide open, and useful for taking up pieces of charcoal in aluminium-plate operations, &c. The pliers,

* Latin *Argentum*, silver, and *fero*, I bear—silver-bearing.
† Fe., short for *ferrum* (Latin), iron.
‡ (Latin) *Alumen*, clay.

which are made of good steel, are now to be utilised as a horseshoe magnet in the following manner:—Left wide open, the pole of a big horse-shoe magnet (the use of which I am sure would be allowed for a few minutes by any enterprising and generous optician in London) marked "N," is to be carefully and squarely drawn down either side (say, that shown in the figure) from the part marked *e* to the tip of the leg beyond *d*: this is to be repeated, say, twelve times. Then the plies are to be turned over, so that the other side is uppermost, and it is to be, in like manner, gently but firmly rubbed by the magnet, always in the one direction, from *e* to *d*, by the other S. pole of the big horse-shoe. The Pyrologist will now possess quite a strong little magnet for testing iron, &c., in minerals, and by closing the legs with the brass collar, as at *a*, no armature is required. The end *c* acts as a charcoal borer or small screw-driver.

I am requested to mention that Messrs. Herring & Co. decline to sell P. acid retail. I get mine from Mr. Wooster, chemist, Broadway, Turnham Green.

THE USE OF THE TRICYCLE.*

BY DR. B. W. RICHARDSON, F.R.S.

I WOULD specially recommend persons who are excessively nervous and of uncertain mind not to use the tricycle. In such people the anxiety attendant on the exercise is injurious,—out of proportion to the service that is gained by it. They are ever on the strain to avoid accident and danger, and ever on the look-out for accident and danger. From these causes they fail to obtain a good command over the instrument. They are not certain what to do when other vehicles meet or pass them; they are not sure how to take a turning; they are in doubt as to the mode of going down-hill, and of resting in going uphill; altogether they are perturbed by the attempts they make beyond the value of the attempt. If, therefore, persons of this nature do not, after a few weeks of fair trial, get over these anxieties, they had better not continue to court them. I would strongly recommend all who have a sense of giddiness or of sinking and sickness, after they have made a little way on the tricycle, to give up the exercise, unless after a short training they find these sensations pass away. Or, if while climbing a hill there is felt a sensation of fulness in the head, with a want of power and precision in managing the machine, I would tender the same recommendation. Again, I would as a general rule recommend those who suffer from the affection called hernia not to become tricyclists; and if they break this rule, I would earnestly recommend them to be moderate in their exercise, and not endeavour to compete with their more favoured comrades.

Paterfamilias is often joked by his young friends that he cannot perform their feats, cannot stand on his head, or give a back, or, as the late John Leech forcibly and famously put it, leap over a walking-stick. For these stiff-jointed inactives the tricycle comes in with great force, if they use it with judgment, and do not trespass too much on reacquired skill. During the late autumn, I accompanied a fellow-rider who, though many years older than myself, could beat me in getting along, and who told me that before he began he was so rigid in muscle and joint, he could scarcely get into the machine. A few weeks' practice had set him at liberty from head to foot with such effect that in walking and riding—for he invariably walked up steep hills, pushing the machine before him—he could average his five to six miles an hour for five or six hours per day, and think nothing of the task.

It would seem, at first sight, that men who are fat and cumbrous are not quite the persons to mount the tricycle; but, if such men are in fair health, they are, after a little careful and judicious training, benefited by it more than any others. It constantly happens that men of this build, while they feel the need for exercise more than the slighter-built sort, are unable to take a proper amount of exercise, because of the great weariness which they experience soon after they have walked even a short distance. The natural result of this easy sense of fatigue is that exercise is given up almost altogether in a great number of cases, while in other cases it is a mere pretence; so that practically a habit is developed which promotes an objection to exercise, and a steady increase of all the dangers which follow upon prolonged muscular inactivity. In this state, they who are affected are apt to follow one of two courses, both of which are bad. They either settle completely down to repose, and attain a form of chronic feebleness, which requires to be provided against by avoiding every kind of lively effort; or, taking sudden alarm at some sensation they have experienced, or some observation they have listened to, they rush into forms of violent exercise, such as climbing mountains, or volunteering, or making forced walks, or such-like efforts. I believe I have seen more mischief induced in the class of persons whom I am now describing, by their attempts to get into condition through the means of excessive exercise and physical strain, than in any other class.

They who court this mode of recovery from their helplessness are of all less fitted to bear sudden strain. In them the muscles are feeble and out of play; in them the muscles, including that most important of all the muscles, the heart, are overloaded with fat; in them the blood-vessels are often weakened, and have lost their natural resilience, if they have not undergone actual change of structure; and in them the breathing organs are in such bad form for extra work, that breathlessness is produced by very little extra exertion. They are, in short, unfit for walking, and they are equally unfit for those extremer measures which are commonly designated as training, or as athletic exercises. To this class of persons, then, if they are not subject to actual disease, organic affection of the heart, the lungs, or the brain, the exercise that may be got from the tricycle is exceedingly useful.

The exercise sought in this manner should not be violent; it should not include attempts to go against time; it should not include attempts to climb steep hills or to run down steep hills at a rattling pace; but it should be taken for some time on level ground, it should be carried on to a point just short of fatigue, and it should be increased little by little each day, until the labour of working accommodates itself to easy habit.

* From "Tricycling in Relation to Health," by Dr. Richardson, in *Good Words* for March.

CHANGE OF HABIT IN ANIMALS.

AS an instance of change of habit in animals, as noticed in your issue of the 3rd inst., with regard to the carnivorous parrot, I may mention that within the last five or six years the baboons of a certain region in the colony of the Cape of Good Hope have developed a flesh-eating taste.

I do not hear that this has become general among the baboons of the colony, but I know that some farmers in the mountainous parts skirting the Karroo plains towards Graaff-Reinet have suffered severely through their depredations among their flocks. Formerly these animals were satisfied with prickly pears and other kinds of fruit, birds'-eggs, locusts, and, as a rare treat, scorpions, which I have seen them busily turning over huge stones to obtain. I have never been near enough to see the process of killing the scorpion, but my husband told me that they most cleverly divested the little reptile of its sting before trusting it between their teeth; but never till of late were they known to kill a sheep or a lamb for food. I have also been told by one farmer that the baboons on his place had killed many quite young lambs, and torn them open, merely to obtain the milk contained in the stomach. Is this preliminary to, or a farther development of, the carnivorous propensity?

As a more pleasing change of habit and adaptation to circumstances in the same part of the world is the fact that a small bird, called by the natives "Tink-tinky," on account of its notes, and by the Dutch colonists, "Kapock (frost) vogel," from its snow-white nest, which, before the introduction of sheep, made use of the silky fibre of the wild cotton as material, now uses the wool which it gathers in sufficient quantities from the Nimosa and Euphorbia thorns, finding probably that it is more easily woven into the thick felt of which the beautiful little bottle-shaped nest is composed, merely placing a little of the silky fibre in the bottom of the nest as a lining.

M. CAREY-HOBSON.

THE "SOUND" OF FISHES.

I OBSERVED some few weeks back (KNOWLEDGE, p. 295) that Mr. Mattieu Williams objected to my naming the "air-bladder" of a fish the "sound," Mr. Williams's contention being that this latter name should more properly be applied to the *aorta* or chief artery of the fish. With all deference to Mr. Williams (who is not a biologist, as "Old Fossil," in your last issue, remarks), I maintain that he is confusing, not two names merely, but two distinct systems of terminology. It is perfectly immaterial to me, as a naturalist, *what* fishmongers or fishermen call the "sound." As a zoologist, I have no concern with the terminology of fisher-folk or of fish-dealers. What does concern my readers and myself, is that I should use names *which are in common use in natural history science*. The name "sound," as a popular name for the "air bladder," has been in use in natural history class-rooms for many years back, and whatever be the justification for its use, I maintain I was entitled to employ it, for the reason just mentioned. The question raised by Mr. Williams amounts to this:—Whether a scientific or a fisherman's use of a particular name is to be held as correct. Personally, I prefer to call the sac in question the "air-bladder"; but I cannot, at the same time, permit Mr. Williams to suggest that my usage of the name is an erroneous practice. "Sound," in fact, is a scientific as well as a popular term. It is not my fault if the meanings attached to it are of varied nature.

ANDREW WILSON.

XUM

ELECTRO-MAGNETIC THEORY OF LIGHT.

IN a recent number of KNOWLEDGE you wrote :—"A correspondent who gives us no name, asks us to explain the electro-magnetic theory of light. We know of no such theory. The writer, who says the undulatory theory is fast being swallowed up piecemeal by the electro-magnetic, knows very little about the matter. The evidence for the undulatory theory is simply overwhelming."

Most will agree in your two last conclusions; but as there *is* an electro-magnetic theory of light, possibly ignorance of its existence on the part of the most omniscient man I know ["Oh Lord, sir! Why there 't serves well again."—ED.], may justify compliance with your nameless correspondent's request for an explanation of it. It is, in fact, an undulatory theory, and only a modification in detail of the theory of Huyghens. Clerk Maxwell suggested the electro-magnetic theory as a means of determining the relation between the phenomena of electro-magnetism and those of light, based upon the assumption that each of these is due to certain modes of motion in the all-pervading "ether" of space, the phenomena of electric currents and magnets being due to streams and whorls, or other bodily movements in the substance of the ether; while light is due to vibrations to and fro in it. Dr. Thompson writes on this subject as follows :—"Here is evidence for thinking that magnetism is a phenomenon of rotation, there being a rotation of *something* around an axis lying in the direction of the magnetisation. Such a theory would explain the rotation of the plane of polarisation of a ray passing through a magnetic field. For a ray of plane-polarised light may be conceived of as consisting of a pair of (oppositely) circularly-polarised waves, in which the right-handed rotation in one ray is periodically counteracted by an equal left-handed rotation in the other ray; and if such a motion were imparted to a medium in which there were superposed a rotation (such as we conceive to take place in every magnetic field) about the same direction, one of these circularly-polarised rays would be accelerated and the other retarded, so that, when they were again compounded into a single plane-polarised ray, this plane would not coincide with the original plane of polarisation, but would be apparently turned round through an angle proportional to the superposed rotation. An electric displacement produces a magnetic force at right angles to itself; it also produces (by the peculiar action known as induction) an electric force which is propagated at right angles, both to the electric displacement and to the magnetic force. Now it is known that in the propagation of light, the actual displacements or vibrations which constitute the so-called ray of light are executed in directions at right angles to the direction of propagation. This analogy is an important point in the theory, and immediately suggests the question whether the respective rates of propagation are the same. Now, the velocity of propagation of electro-magnetic induction is that velocity "v," which represents the ratio between the electrostatic and the electro-magnetic units, and which (in air) is believed to be $2{\cdot}9857 \times 10^{10}$ centimètres per second. And the velocity of light (in air) has been repeatedly measured (by Fizeau, Cornu, Michelson, and others), giving as the approximate value $2{\cdot}9992 \times 10^{10}$ centimètres per second. The close agreement of these figures is at least remarkable. Amongst other mathematical deductions from the theory may be mentioned the following :—(I.) all true conductors of electricity must be opaque * to light; (II.) for transparent media, the specific inductive capacity ought to be equal to the square of the index of refraction. Experiments by Gordon, Boltzmann, and others, show this to be approximately true for waves of very great wave-length. The values are shown below. For gases the agreement is even closer—

	K.		N^2
Flint Glass	3·162	.....	2·796
Bisulphide of Carbon...	1·812	.....	2·606
Sulphur (mean)	4·151	.....	4·024
Paraffin.....	2·32	.....	2·33

A. K. ROLLIT.

* In some crystalline bodies which conduct electricity better in one direction than in another, the opacity to light differs correspondingly. Coloured crystals of Tourmaline conduct electricity better across the long axis of the crystal than along that axis. Such crystals are much more opaque to light passing along the axis than to light passing across it. And, in the case of rays traversing the crystal across the axis, the vibrations across the axis are more completely absorbed than those parallel to the axis: whence it follows that the transmitted light will be polarised."

OUR ANCESTORS.

MR. GRANT ALLEN, at p. 351 of KNOWLEDGE, in describing Palæolithic men, says their flint implements are found buried under the concreted floors of caves. This no doubt is true, but he goes on to say that, "since the days of Palæolithic men, Britain has been submerged beneath several hundred feet of sea." Surely Mr. Grant Allen can hardly expect your readers to accept this statement. That Great Britain may have been submerged since the earlier Palæolithic men lived upon what is now Britain is, perhaps, possible, but that Britain has been under the sea since the Palæolithic men of the caves lived here is, I think, a stretch of the writer's imagination. Close to where I live in London, implementiferous gravels and sands are common, in one great deposit of loam and sand at Stoke Newington, Palæolithic implements are only four feet or less beneath the surface, and the surface is loam or sand full of fresh water shells, with no trace whatever of the sea in any part. These Palæolithic deposits have never been under the sea. Mr. Allen there describes the men as low-browed, fierce-jawed, crouching creatures, inferior to the existing Australians or Andaman islanders, &c., rather a complete and sweeping description, when it is remembered that no complete skeleton of any individual of Palæolithic times has yet been seen, and not enough scattered bones are known sufficient to build up a single skeleton.

I should feel inclined to question the correctness of the greater part of the paragraph in which the above statements occur, but however low-browed and fierce-jawed these "black fellows" may have been, I can make one statement about the works of the Palæolithic men who lived where north-east London now is, that few people will be inclined to deny who have ever seen the objects I refer to, and that statement is, the flint implements the Palæolithic men have left behind them here, are as perfect works of art in their way as anything ever made by the best artificers of ancient or modern times. The implements found here are often models of symmetrical beauty, indicating eyes perfectly trained to the appreciation of true curves and beautiful and appropriate forms. The workmanship of these tools shows the work of skilful and delicate fingers, workmanship that to the present day simply defies the efforts towards imitation of the most accomplished modern forgers.

That the time is immensely distant since the older Palæolithic implements were made, and that the older tools are generally very rude, I quite admit, but Palæolithic times lasted for a long period, and the gravels containing the largest number of Palæolithic implements have certainly never been under the sea since the implements were made. WORTHINGTON G. SMITH.

INTELLIGENCE IN ANIMALS.—About three years ago I was living in a street in Birmingham. At the back of the houses ran strips of garden, or half garden and half courtyard, separated from one another at the further end by party walls about 4 ft. 6 in. high. The grating over the drain in our court was broken, and rats once or twice paid us a visit, and once the cat vigorously pursued one all over the court. The cat was not full grown and did not manage to catch the rat, which retired out of the cat's reach beneath a water-butt raised on single bricks. Pussy watched and waited for awhile, but obtaining no help from human friends, suddenly jumped up and dashed over the party wall and in less than two minutes returned full speed with another cat, eager *both* of them apparently for a renewed chase. Unfortunately the rat had meanwhile discreetly retired to the drain. A cat which I had in Birmingham, and another which I have now, both entirely untaught, have a trick of rattling the handles of the doors with their paws when wishing either to enter or leave the room. When *in* the room *with us* she, after pawing the handle, looks round beseechingly to us.—A. M. F.

REASONING IN ANIMALS.—Many most interesting cases have been given in KNOWLEDGE on this subject. Will you allow me to supplement one which, I think, surpasses all yet given. One of our seamen took a large, white-coated bitch at Petropauloski, in Kamschatka, and she became a great favourite with the crew. Subsequently the ships were ordered to China, and lay off the English Hongs at Canton. It was customary to allow the bitch to swim ashore for a run, and she used to swim back to the ship afterwards, and be hauled on board by placing her neck and fore-legs in a bowling-knotted noose. But the tide running furiously in that part of the river, the dog had no chance of making the vessel by pushing off from the shore ahead of the ship, but used to come down to a low wharf opposite, and watch for bits of wood, &c., floating, to see which way the tide ran, and then running up in a contrary direction, would jump in, and come down with the tide to the ship. On one occasion she was seen to watch for some time; but, as it happened, nothing floated by to give her an indication of the direction of the tide. Whereupon, she was observed to drop down on one foreleg, and, the planks of the wharf being almost level with the water, hang the free paw over into the water, by which means, apparently, she obtained the knowledge of the tide's direction, and, running up against it, came off, as usual, quite safely. She was one of the finest water dogs I ever saw. I well remember her jumping off our gangway at Aden, 16 feet from the water line.—PERCIVAL A. FOTHERGILL, B.A., F.R.A.S.

Letters to the Editor.

[The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with their writers. All communications should be as short as possible, consistently with full and clear statements of the writer's meaning.]

All Editorial communications should be addressed to the Editor of KNOWLEDGE; *all Business communications to the Publishers, at the Office, 74, Great Queen-street, W.C.*

All Remittances, Cheques, and Post-Office Orders should be made payable to Messrs. Wyman & Sons.

**** All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.*

All Letters or Queries to the Editor which require attention in the current issue of KNOWLEDGE, *should reach the Publishing Office not later than the Saturday preceding the day of publication.*

(I.) Letters to have a chance of appearing must be concise; they must be drawn up in the form adopted for letters here, so that they may go untouched to the printers; private communications, therefore, as well as queries, or replies to queries (intended to appear as such) should be written on separate leaves.

(II.) Queries and replies should be even more concise than letters; and drawn up in the form in which they are here presented, with brackets for number in case of queries, and the proper query number (bracketed) in case of replies.

(III.) Letters, queries, and replies which (either because too long, or unsuitable, or dealing with matters which others have discussed, or for any other reason) cannot find place here, will either be briefly referred to in answers to correspondents, or acknowledged in a column reserved for the purpose.

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

"God's Orthodoxy is Truth."—*Charles Kingsley.*

Our Correspondence Columns.

VEGETARIANISM. — PLANTS IN BEDROOMS. — "THE BURIAL OF MOSES."—DESTRUCTION OF SODOM AND GOMORRAH.

[326]—I must thank Sir Henry Thompson for a personal reply, which is as welcome as it was unexpected. In a discussion on vegetarianism that has been recently going on in the *Echo* evening newspaper, his name, and that of Dr. Richardson, have been definitely quoted (among those of a number of obscurities) as advocates of a purely vegetable diet. Now it can matter little to the public, and assuredly need not influence it, to learn that Mr. Smith, of Hoxton, has brought up a large family on French beans and cabbages, or that Mr. Brown, of Soho, consumes (and believes in) "The Nutriment of Longevity," which he himself sells; but the case is very different when it is definitely predicated by men of world-wise eminence, like Sir H. Thompson and Dr. Richardson, that they are, in effect, vegetarians. I may say that before penning the letter (285, p. 362) which has elicited Sir Henry Thompson's valuable rejoinder, I had learned privately that both himself and Dr. Richardson did, as a matter of fact, employ a mixed diet themselves. This being so, I thought that a proclamation of the truth would enable all interested in the food question to estimate the *bonâ* (or *malâ*) *fides* of the vegetarians who quote two distinguished physiologists as converts to, or advocates of, their system, and to appraise the exact worth of the argument which they profess to derive from such authority.

I will tell "A Fellow of the Chemical Society" (letter 315, p. 40) one way in which I believe the wretched potato diet does "influence Irish character." It may be gathered from the reports of the hideous outrages which unhappily reach us daily from Ireland, the details of which show, beyond cavil, that their perpetrators are the most arrant curs upon the face of the earth. Firing from safe concealment behind stone walls, and running like hares the moment their barrels are emptied; breaking, in overwhelming force, disguised, and with blackened faces, into lonely houses, and shooting and maiming solitary and defenceless men and women; skulking away at the mere sight and sound of resistance, and never daring to face a human being who is in a position effectually to oppose them, these people present an example of an arrant cowardice which happily disgraces but few (so-called) "civilised" races in the world. Now, enlist these men, give them their daily ration of good, wholesome meat, and what do we find? Simply, that there is no braver soldier in existence than the Irishman. Where are there finer regiments to be found than those mainly recruited from Ireland? Why, I have myself known an Irish private, totally unarmed, go in and seize a comrade half frantic with drink and armed with a bayonet with which he had previously kept several men at bay. As an example of cool courage, uninflamed by the excitement of action, this seemed very striking to me, and certainly in odd contrast to the pusillanimity of his vegetable-fed *confrère*. Finally, I would say that I am by no means convinced that the chemical hypothesis of nutrition is invariably sustained by experience. I can remember when Liebig's theory was regarded as being as unassailable in its entirety as the Apostles' Creed. Is this the case now? Growing plants, I may tell "J. C. L." (query 288, p. 410), are unhealthy in bedrooms from the fact that at night they give out carbonic acid. In sunlight, on the other hand, they exhale oxygen and keep the air of a room pure. In reply to his next query (289, same page), I fancy that he will find the poem he inquires for in "Maunder's Class-book," also in the "National Reading Book," Book V., published by the National Society, Westminster.

"A. N." (query 303, p. 410), may rest assured that no scientific proof whatever exists that Sodom and Gomorrah were destroyed on July 31, 1898 B.C., either by aerolites or in any other way.

A FELLOW OF THE ROYAL ASTRONOMICAL SOCIETY.

TELESCOPES.

[327]—While fully endorsing the opinion given by H. Sadler in KNOWLEDGE, p. 389, as to the advantage of a silvered glass reflector, I beg permission to add that by means of a tin cap, with a handle like the lid of a saucepan, fitting closely on the cell of the speculum, the film may be preserved, if not in its original beauty, yet in a perfectly serviceable condition for so long a time, that the expense of renewal will become a matter of comparatively little importance. It must be remembered, however, that this involves either an opening in the side of the tube large enough to admit the hand with the lid (which might be jointed for the purpose) on the removal of the mirror when not in constant use. Circumstances have obliged me to follow the latter plan; but I find no great inconvenience in it, and am convinced that a little ingenuity would supply a mode of effecting it readily, and at a trifling expense. With due precaution, the adjustment need not suffer.

T. W. WEBB.

NIGHTS WITH A THREE-INCH TELESCOPE.

[328]—Could you at the end of these articles give the constellation and letters of the stars which will be described in the following article? I make this suggestion in order that amateurs, like myself, may afterwards compare what they have observed with the article in your succeeding number.

F. C. B.

THE POTATO.

[329]—In No. 12, Jan. 20, "F. C. S." gives an article on "Something About the Potato," which contains several statements which I had hoped to see flatly contradicted by other and abler hands than mine.

He says, "the influence of the blossoms makes a great difference," and quotes a case showing an increase of about 13 per cent. where these are removed. Let "F. C. S." try a small plot during the coming season, and give us his results. With Victorias, which are about the freest bloomers known, the writer has several times tried it on plots of from one-tenth to half an acre, with no appreciable difference; in fact, if anything, the result was against the mutilation of the plants.

Further on he says, "in frozen potatoes the sugar is doubled and the starch diminished." This, I fancy, happens after or at the time of the frost going out, as I find a frozen potato, if plunged into hot wood-ashes and there cooked, is quite dry and floury, and has none of the sweetness peculiar to a frosted potato.

Again, he says "compost has no effect unless to increase the proportion of starch." It must surely have been poor compost. Please say what was its composition, and the amount of increase of starch?

Again, he says "potatoes grown on moist soils, and soils containing much organic matter, are most liable to disease;" while further on, he says "the unmanured plots are highest in disease." The one statement is a contradiction of the other, and contrary to any result I have ever heard of any one attaining, and certainly far from the results attained by myself.

Further on, he says "Good peat is found to give even better results than good stable manure." This would certainly be good peat, and a mine of wealth to its owner. Will "F. C. S." quote his authority on this matter, as I consider it downright nonsense?

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Can "F. C. S.," or any other reader, say what is the difference of starch in a soft, waxy potato and a dry, floury one; and give any reason for the difference, other than sunlight and a minimum of moisture? FARMER.

HOG PUZZLE.

[330]—Lieut.-Col. W. H. Oakes' solution (see letter 288, p. 363) is the following:—We have to find four pairs of squares having a common difference. This common difference will be:—

Firstly. A multiple of 21.

Secondly. The product of four consecutive terms of an increasing arithmetical progression.

Thirdly. The product of each of four pairs of factors.

Now $21=3\times7$. Also, 1, 3, 5, 7, is the lowest arithmetical series of four terms that includes 3 and 7. Also $1\times3\times5\times7$ is the product of each of four pairs of factors, namely, 105×1, 35×3, 21×5, and 15×7. The product of this series gives the lowest number of shillings that satisfies all the requirements of the problem. Therefore, each husband has spent 5 guineas more than his wife.

N.B.—Half the sum and half the difference of each pair of factors will give the number of hogs bought by each man and his wife; so we get 53 and 52, 19 and 16, 13 and 8, and 11 and 4.

[Correctly solved also by J. A. Miles, C., and J. R. Campbell.—ED.]

The original puzzle (see letter 198, p. 232) admits of the following arithmetical solution:—

The number of shillings in 3 guineas is 63. We have, therefore, to find 3 pairs of squares with a common difference of 63. Now, 63 is the product of 63×1, 21×3, and 9×7, Half the sum and half the difference of each of these pairs of factors will give the number of hogs bought by each man and his wife, namely 32 and 31, 12 and 9, 8 and 1. By question, H bought 32, and K 9; also E bought 12, and G 1. Therefore, H and A, E and K, C and G, are the respective husbands and wives.

William Emerson published the original puzzle and its algebraical solution nearly 120 years ago. I append his solution as he gives it.

Let	1	x = hogs some man bought, $x-y$ = wife's hogs.
per q^n	2	the money for the man's $=xx$.
	3	$xx-2xy+yy$ = wife's money.
	4	$xx=xx-2xy+yy+63$.
4 tr.	5	$2xy=63+yy$.
$5+$	6	$x=\frac{63+yy}{2y}$ = whole number.
$6-y$	7	$x-y=\frac{63-yy}{2y}$ = whole number.
But	8	In this case y must be an odd number, $=1, 3, 5, 7$, &c., but it cannot be 5.
8	9	If $y=1$, $x-y=31$, $x=32$. $y=3$, $x-y=9$, $x=12$. $y=7$, $x-y=1$, $x=8$.
per q^n	10	A has 32 hogs, and Q 9, Also B has 12, and P 1.
	11	Whence B and Q C and P } are man and wife. A and R

HERBERT REES PHILIPPS.

[331]—Let x = the number of guineas each husband laid out more than his wife $=21x$ shillings.

Call the four men A, B, C, and D, and their four wives a, b, c, and d.

Let p = the number of hogs A bought more than a.
" $q=$ " " B " " b.
" $r=$ " " C " " c.
" $s=$ " " D " " d.

Now, by the conditions of the question, p, q, r, and s, are four consecutive terms of an increasing arithmetical progression. Therefore it follows that p is less than q,

q is less than r, &c.

But the number of hogs bought by each man is equal to the number of shillings given for each hog, therefore—

p hogs cost p^2 shillings,
and q hogs cost q^2 shillings,

and as by the question—

$$p^2=21x,$$
$$\text{and } q^2=21x,$$
$$p^2=q^2,$$
$$\text{and } p=q,$$

but it has been shown that p is less than q.

Therefore, as things which are equal to one another cannot be greater or less than one another, the question is an impossible one.

J. A. MILES.

THE PERFECT WAY IN DIET.

[332]—When a Fellow of the Royal Astronomical Society (285) writes of the abolitionists of the barbarism of the slaughter-house as being "as weak numerically as they are intellectually," is he really ignorant of the facts (1) that to all intents and purposes two-thirds of the human species are, and always have been (at all events, since the times when they emerged from the universal primæval barbarism), *nolentes volentes*, abstinents from flesh-meats: that it is the richer classes in all communities alone who support the slaughter-house; while the poor, because of the selfishness of the rich, are starving upon the minimum amount of non-flesh foods (upon badly-cooked potatoes and cabbages, it may be); (2) that there are such names in history as Pythagoras Sakya-Muni (the founder of a religion the most philosophical and most humane, in its essential doctrine, that has ever been preached on the earth, and which has some 300,000,000 followers), Plutarch, Seneca, Porphyry (the most erudite philosopher of antiquity), Clemens of Alexandria, Chrysostom, Gassendi (whom Boyle characterises as "the greatest philosopher among scholars, and the greatest scholar among philosophers"), Mandeville, Evelyn (*Acetaria*), Ray, Linné, Halley, Cheyne, Voltaire, Howard, Wesley, Rousseau, Franklin, Shelley, Graham, Hufeland, Struve, Daumen, Lamartine?

Before this critic attempts again to pronounce upon the value of a creed which has engaged the earnest attention, and in very many cases the entire approbation, of the most profound thinkers of all the best times, let me exhort him to study, *with some attention*, at least, such writings, *e.g.*, as Plutarch's "Essay on Flesh-eating" (Περι της Σαρκοφαγιας), the most remarkable ethical production of antiquity; Seneca's "Letters," &c.; Gassendi; Shelley's "Essay"; Professor F. W. Newman's "Lectures"; and last, not least, Dr. Anna Kingsford's "Perfect Way in Diet."—HOWARD WILLIAMS, M.A.

UNIVERSITY OF LONDON MATRIC. EXAM., JAN., 1882.

[333]—"Out of 800 candidates, less than 300 passed." Ought not this result to lead to some inquiry as to the manner in which the examiners performed their duty? From the Arithmetic and Algebra paper given by Dr. John Hopkinson and B. Williamsen, Esq., M.A., I wish to place two questions before your readers for their consideration.

No. 5 question is the following:—"Six terms are in arithmetical progression, and also in geometrical progression, and their sum is 54. What are they?" The only solution appears to be that which we find in the multiplication table:—"Six times 9 are 54." No. 9 question occupies nine lines, and is as follows:—"Suppose that gold is worth 15 times as much as silver, and that silver is worth 100 times as much as copper. Find the proportions of the metals in a certain coin worth 4s., having given that a coin with double as much gold, the same quantity of silver, and 5 times (or 5 million times?) as much copper, would be worth 7s. 9d.; a coin having 5 times as much gold, the same silver, and twice as much copper (?) would be worth 19s.; and lastly, that a coin with the same gold, double the silver, and one-half the copper (a half-millionth would do equally well) would be worth 4s. 3d." If we construct the equations, and from (4) subtract (1), and next multiply (1) by (2), and from the product subtract (2), the results will be found inconsistent, unless we assume that the quantity, or rather "proportion," of copper is *nil!*

Will you, sir, or some of your readers, characterise questions such as these? If the object is to baffle and bewilder candidates, by "rejecting" 500 out of 800, the examiners are shown to have attained their end. But will the London Matric. Exam. continue to stimulate the acquisition of "knowledge," if conducted on this principle?—A TEACHER.

MATHEMATICAL PARADOX.

[334]—The following mathematical paradox is the shortest, and at the same time the most difficult, I have met with. It seems to have seriously perplexed even so accomplished a mathematician as Lacroix. I should like to hear the opinions of some correspondents before giving my own.

$$(+a)^2=(-a)^2$$
$$\therefore 2\log.(+a)=2\log.(-a)$$
$$\therefore \log.(+a)=\log.(-a)$$

a result which is not true. S. L. B.

INTELLIGENCE OF A MONKEY.

[335]—One of the tricks we were in the habit of playing him was this: he had a cord suspended from the ceiling, with a loop just large enough for him to get through; then one of us passed his chain through the loop something like a dozen times in different ways; we then watched the result. After getting into the loop and

stretching it open, he would, with his disengaged hand, take up the chain and look which way (as he thought) it passed through, and, by following the winding of the chain, would speedily untwist the whole, to his utmost delight, and to the astonishment of those who witnessed it. You can imagine the intelligence required when you think of the difficulty overcome. He had to go outside the loop every other turn.—F. SELBY.

PERSONAL ILLUSION.

[336]—To me, horizontal lines appear much plainer than perpendicular lines. Thus, I can tell the time by a clock more easily at, *e.g.*, 3.45 than at 6. So, also, the letters of printed words in small type seem to touch each other; but, if I turn the page sideways, they open out. JUMBO.

EXPLOSION OF AMMONIA.

[337]—The explosion of aqua-ammoniæ, related in KNOWLEDGE of Feb. 10, reminds me of a similar occurrence in my own experience. A bottle of ammonia, having the stopper tied down, had remained undisturbed for some time in my photographic workroom. During some hot summer weather it burst with a loud explosion, and the contents were scattered in all directions. The heat of the weathes, no doubt, liberated a quantity of ammonia gas, which, being unable to escape, shattered the bottle. In the account of the accident detailed in KNOWLEDGE, the bottle of ammonia is stated to have been kept on the mantelpiece, and to have exploded in the woman's hand. A mantelpiece at this time of the year is presumably a very warm place, and if the stopper of the bottle had become fixed (as often happens), it is not difficult to understand how the heat of the woman's hand may have proved "the last straw," the bottle having probably been almost at bursting point from the pressure of ammonia gas liberated by the warmth to which it was exposed on the mantel-piece. It is impossible to suppose that an aqueous solution of ammonia could be explosive in the usual sense of the word, and I think the above must be the true explanation of the accident, and a source of danger which perhaps few are aware of. B. A. (Cantab.)

HOW TO PRESERVE FOSSILS, BONES, &c., FROM THE ACTION OF THE AIR.

[338]—1. See that the fossils are washed free of all efflorescence, dirt, &c., and well dried. 2. Take strong Scotch glue 4 oz.; dissolve in glue-kettle, with enough water to make a thick mucilage. Then add 1 oz. of boiled linseed oil, stir well; dissolve ¾ oz. of bichromate of potass in a small quantity of water, and add this to the glue, then thin down with water to the consistency (while hot) of milk. 3. Put the dried fossils or bones into an oven and make them very hot, then drop them into the boiling solution and keep them near the simmering point for half-an-hour. Wipe them with a sponge and expose them to dry in the air and sunshine. If properly managed they will now keep indefinitely.

P.S. The above prepared glue, of the proper thickness, is the best thing for repairing broken fossils, and for making any joints required to be strong and good. F.

A SERIES OF ASTONISHING COINCIDENCES.

[339]—In connection with the editor's papers on chance and coincidence, the following remarkable instance of purely fortuitous coincidence will be of interest. About a month since a friend of mine, Mr. Hunt, went to a small town, about twelve miles from Exeter, to attend a meeting, returning home the same night. While there he called upon the Rev. J. Smith, remaining about an hour. Just before leaving, the postman called at the house, and a letter was brought in addressed "Mr. J. P. Hunt, C——." The servant said to her master, "Please, sir, the postman wants to know if you know who this letter is for, as he does not know the name in the town." As it was distinctly addressed to my friend, he opened it, and found it was from a chum of his in the North of England. I may say that the letter contained nothing whatever of importance. A few days afterwards another letter arrived (at his proper address) from the same fellow, saying: "I wrote you last week, but, of course, you never got the letter, for, like an ass, I directed to C——, of all places in the world." I should say that Mr. Hunt had nothing whatever to do with C——. The postman knew nothing at all about him, and he was not expected at Mr. Smith's. So that the purely fortuitous character of the circumstances that guided the misdirected letter to his hands is complete. H. SNELL.

BRIGHTNESS OF TELESCOPIC IMAGES.

[340]—In a review of Professor Newcombe's work in KNOWLEDGE, the common opinion that large telescopes increase the brightness of objects having visible surfaces is stated to be erroneous. Is this so in all cases? Let the brilliancy of the retinal image of (say) the moon be unity. Then, with a telescope having thirty-six times the light-gathering surface, the image at the focus is thirty-six times as bright. If I magnify this image nine times superficial, the magnified image is one-ninth as bright; but the image at focus is thirty-six times as bright as the retinal image. Therefore, the magnified image is thirty-six ninths, or four times, as bright as the retinal image. If I increase the aperture and retain the same magnifying power, the image should be brighter still. No doubt I am astray somewhere, and should esteem it a favour if you would kindly point out where. Thanking you for past favours. W. KIDD.

[You will find that where magnifying power is less than increase of illuminating power, the eye can only receive a portion of the emergent pencils. My friend Dr. Huggins had an old telescope intended to give great increase of light (a night glass), and he found the emergent pencils were an inch in diameter—as, he said, "a telescope for a horse, not for a man."—ED.]

THE OWNER OF THE CORONA.

[341]—I was present at the birth of the nursery rhyme to which you refer in your last number, and can vouch for the original form in which it appeared. The part in question ran thus:—

[Our correspondent must excuse our omitting the first two lines and the last, which are to some degree personal.—ED.]

. . . . . .
. . . . . .
He thought himself owner of half the Corona,
. . . . . .

The rhymes were made by Dr. Thorpe, a member of the Sicilian Eclipse Expedition of 1870. On leaving Poretta, a station between Verona and Florence, Professor Clifford commenced making nursery rhymes on the names of the stations taken from Bradshaw, and Dr. Thorpe carried on Professor Clifford's idea by rhyming the names of the members of the expedition. There is a moderation about the original recension which recommends itself to my judgment. It will be noticed that the claim set up is only to the ownership of "half of the Corona." The verses were received with great applause by the members of the Eclipse Expedition, who had been travelling with the "owner" for some days. Mr. Rand Capron's version must have been derived from an inaccurate source—perhaps the owner subsequently saw his way to improving them.

A MEMBER OF THE SICILIAN ECLIPSE EXPEDITION.

INTERIOR HEAT OF THE EARTH.

[342]—Your correspondent, "B." (No. 287), will find, if he refers to the Rev. Osmund Fisher's recently-published work on the "Physics of the Earth's Crust," that Professor Mohr's speculations have not escaped the notice of scientific men in this country. He will also find his deductions from the observations made in the bore-hole at Sperenberg very ably combated by Mr. Fisher in the above work.—I am, your obedient servant, D. C. W. HINE.

EARTH'S INTERNAL TEMPERATURE.

[343]—I have not seen Professor Mohr's book, but I recollect the inference he attempted to draw from the temperature observations at the Sperenberg borehole. It is altogether fallacious, and the error has been pointed out by Professor Everett in the "Report of the Committee of the British Association on Underground Temperatures," and by the Rev. O. Fisher in his work on the "Physics of the Earth's Crust." In almost any book on geology—say, "Coal: its History and Uses" (Macmillan), "B." will find an account of the result of the examination of coal plants by competent botanists.

A single instance of asphalte in granite is rather slender evidence for disputing the generally-received views as to the origin of that rock. It may have been a case in which granite has been formed by the intense metamorphism of a rock containing bituminous matter, and if the change was effected deep down under ground, the pressure may well have prevented volatilisation and escape.—A. H. G.

THE RAINBAND.

[344]—I observe in a recent number the discovery of this band in the spectroscope is given to Piazzi Smyth, which may be true, as there is no date affixed, but if so it must have been before April 20, 1871. On that date Professor James Cooke asked me if I had noticed

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the dark line which preceded rain; and, producing a pocket spectroscope, pointed it out. Having purchased, the year before, when at the British Association in Liverpool, from John Browning, a six-prism automatic spectroscope, Mr. Cooke came home with me to test therein this band, which was now very obvious, and from that day to the present it has been, before rain, a familiar subject to my friends and myself.

Referring to your foot note of the nursery rhyme, which, doubtless, Mr. Lockyer enjoyed as much as anyone (?), these are the words given to me by one of the members of the 1870 Eclipse Expedition, on his return home:—

* * * *

Till he of the Corona,
Says, Norman is owner,
And the rest of the sun shines for Lockyer. PRIAM.

EARTHENWARE INSULATORS.

[345]—*Apropos* of the electric "Telegraph," "The wires and insulators," KNOWLEDGE, Vol. I., 374, I was, some four or five years ago, having a conversation with an electrical engineer; and he informed me that in the manufacture of earthenware insulators, a large quantity of animal charcoal was used; but if any human bone got mixed with the others and burnt, the insulators (so-called) were useless, as they did not produce insulation, and he told me that he was a loser in a large contract entered into for insulators, in consequence of some human bones having got among others which were used for the charcoal for mixing with the other materials of which the insulators were composed, and not one of which executed its appointed task. Have you ever hear of this peculiarity of human charcoal, and can you account for it? A. T. C.

SPECIAL NOTICE.

FOR some time past our Queries and Replies have involved a serious and growing difficulty, which we had proposed to remedy in some degree by having two numbers of 32 pp. per month. But the difficulty increased so much that this measure would only, we feel assured, afford temporary relief. We must, therefore, adopt a more satisfactory remedy. The difficulty is this: many questions are asked (perhaps about a tenth of those asked appear under head Queries); to some questions ten or a dozen replies are sent in, of which, perhaps, only one can be admitted, while other questions remain unanswered. Correspondents who have written out queries or replies are not well pleased if their labour is wasted, yet much labour must be wasted according to our present arrangement. If, as we proposed, we enlarge our sheet occasionally to admit more questions and answers, there would be delay in many cases, and the bulk of our readers would not care greatly to have simply eight more pages of correspondence, queries, and replies. In fact, we should have not a few letters pointing out that whereas in number, x, 12 pages out of 20, or 3-5ths, were original matter, in number $(x+1)$ there were 12 out of 28, or only 3-7ths, "and this, Mr. Editor, is grossly unjust." We shall hereafter adopt, therefore, a different system with queries. They will be classified, and sent to experts in the departments to which they respectively belong, who will reply to them at greater or less length, according to the nature of the queries, but in such a way that each reply will convey information to others besides the querist. These replies may, from time to time, furnish occasion for correspondence, corrections, suggestions, and so forth; but, for the most part, a query once asked and answered will be finally disposed of. Thus, much less space will be occupied by questions and replies, while much more satisfactory information will be given not only to each individual querist, but to our readers generally. The "Queries" and "Replies" columns will thus be practically merged in "Answers to Correspondents," classified under various headings—Astrology, Geology, Chemistry, Botany, Entomology, and so forth. It is hardly necessary to point out to our readers that this arrangement, by which the usefulness of KNOWLEDGE will be greatly increased, will not be altogether so inexpensive as one by which readers are left to answer each others' questions. The proprietors of KNOWLEDGE cannot, therefore, *at the same time*, enlarge the numbers. But when the growing circulation of KNOWLEDGE justifies that course *also*, readers will gain much more by it, as there will be an increase of original matter, instead of a mere growth of the Correspondence columns.

OWING to the illness of engraver, the large picture of the looped path of Mars, with reference to the earth, from 1875 to 1892, has been delayed. It will appear without fail next week.—ED.

IT should have been mentioned that the note on the appearance of Jupiter, in No. 18, was extracted from the *Kansas Science Student*.

THE EDITOR.

Queries.

[311]—HEATING ROOM.—Will Mr. W. Mattieu Williams kindly say what are the objections (if any), in a sanitary point of view, to heating a room by means of an atmospheric (Bünsen) burner without a stove-pipe or vent, provided it is kept burning with a perfectly blue flame?—J. W. B.

[312]—PYROLOGICAL.—Will Colonel Ross please say:—1. If, having tried Fletcher's lamp for burning solid fats (modified form for travellers), he can recommend same? 2. Where the 10s. 6d. microscopes he mentions in "Pyrology" are to be got? 3. Where the spectrum lorgnette is to be got, and the price? 4. What fat, or fats, are best for blowpipe work?—JEMIMA.

[313]—BOTANICAL.—Would any one kindly give the name and price of a book giving the derivations and meanings of the various botanical terms?—JEMIMA.

[314]—EYESIGHT.—One of my eyes is only half the focus of the other; what is the best thing to be done?—JEMIMA.

[315]—ASTRO-PHOTOGRAPHY.—Will a reader kindly refer me to a work on this subject, or give a few elementary instructions adapted for a 3¼-inch refractor.—C. J. C.

[316]—ATLANTIC CABLE.—Where can I obtain an account of the laying of the Atlantic telegraph cable of 1865?—W. Y. N.

[317]—SEAL FISHERIES.—How are seal-skins shipped to England, &c., procured? Is it the case that the seal is flayed while alive and conscious, and, if so, how is the animal afterwards treated?—J. H. B. FLETCHER.

[318]—VEGETARIANISM.—I should be obliged if "A Fellow of the Chemical Society" would tell me where to obtain information as to the details of a proper vegetarian diet, so that I could give my household sufficient variety; and also that I might not err, as did those who fed "the prisoners" referred to.—G. A. S.

[319]—BINOCULAR MICROSCOPE.—When using the quarter-inch with binocular microscope, both tubes are not fully illuminated, but there is always a dark shadow in one or other, wherever I place the mirror. Is this unavoidable, or what will remedy the defect?—J. E. S.

[320]—BOTANY.—I am about to commence the study of botany, and should be very thankful for any information as to books, &c. Would Cassell's lessons in the "Popular Educator" be of any use to me? I cannot afford anything that is expensive, as I am only a weaver lad.—F.

[321]—BOTANY.—Providing the ovules in the ovary of a flower were unimpregnated, would they continue to develope, and ultimately to all outward appearance simulate true seeds. Of course, I know they would not germinate. This is against all laws of physiology. But everyone knows that a hen will lay eggs without the visit of the male bird. In replies, please quote references?—JUMBO.

[322]—METEROLOGICAL.—(1) What is the best self-registering rain-gauge, price, and where procurable? (2) Also the handiest cards or sheets for tabulating the readings of the barometer, wet and dry bulb thermometers, rainfall, and prevailing winds? Observations taken thrice daily.—G. B.

[323]—SHOCKING COIL.—Would any reader give full particulars for making a powerful shocking coil, with the best form of battery to use with the same in a small room, as I am desirous of making one.—IN AMBIGUO.

[324]—ELECTRIC ORGAN.—Having an intention of constructing an organ similar to the one at Messrs. Maskelyne and Cooke's, will some reader kindly explain the action, say from the keys to the pipes?—ROBERTO.

[325]—STONEHENGE.—Can you inform me whether the Druids placed the stones there, and if so, by what process?—SUBSCRIBER.

[326]—ANTARCTIC REGIONS.—Required information respecting the geology of these regions. There are volcanoes, but are there stratified rocks? I remember some years ago hearing it stated that elephant's tusks had been found, and I notice in the map an "Elephant Island." Is there any book that would throw light on the subject? I addressed a query to you, which you published, about Valley-terraces in Dorsetshire (No. 261), but no one has thought it worth while to reply.—S. H. W.

[327]—FIRST B.SC. EXAM. (University of London).—Could any reader of KNOWLEDGE tell me the best books to procure for the preparation of the various subjects required in the above?—UNDERGRADUATE.

[328]—SUB TEGMINE FAGI.—Can Mr. Grant Allen tell me why herbage does not grow as readily under beeches as under other

trees? Am I right in thinking that grass under beeches is often of a finer blade than usual? Why does Mr. Allen exclude yellow at page 21 of "Evolutionist at Large?" it surely predominates at least in spring.—M. McC.

[329]—PHOSPHORESCENCE OF FISH.—What is the cause of the luminous appearance, so striking in the dark, observable on the inside of a haddock, chiefly about the bones, even after it has been cured?—LEONARD B. P.

[330]—CLIMBING PLANTS.—In our hemisphere, and in our climate, of course, the sun rises on our left and sets on our right hand. Climbing plants, such as the hop, in consequence, it is believed, of the action between them and the sun, wind round their supports in the same direction. Perhaps some of your botanical readers will tell whether, in the southern hemisphere (say at the Cape or Australia), as the sun rises on our right and sets on our left, these plants wind round in a similar direction, following the sun, and thus in a totally different manner from those with us. Under the equator, might they not be sometimes puzzled as to the direction they ought to take?—W. P. B.

[331]—EVOLUTION AND GEOLOGY.—Dr. Wright, F.R.S., in a lecture at Cheltenham a few days ago, declared the Lias formation to be "the greatest possible stumbling-block to the theory of evolution, for he defied anyone to find in this complete geological chapter the slightest sign of any intermediate steps of life. If palæontologists could determine such steps, the world would soon be convinced of the truth of evolution; but in endeavouring to do this, they would find the zones of life in the Lias a very hard nut to crack." What reply do evolutionists make to this objection?—EUPTERIS.

[332]—MOSSES.—Can anyone recommend me a book giving the English as well as the botanical names of British mosses? Hobkirk's "Synopsis," and Dr. Braithwaite's "Flora" give only the latter.—EUPTERIS.

[333]—SAKKARA TABLET.—Will "A Member of the Society of Biblical Archæology" kindly let me know when the Sakkara tablet, mentioned March 3 on page 379, was discovered, and by whom? What kingly dynasties are mentioned in it, and where is Maspero's correction of these dynasties to be obtained and read?—EGYPTOLOGIST.

[334]—JUPITER.—I should be glad of any information respecting cause and nature of the red spot on Jupiter, also where to find best description of same.—A. H. M.

Replies to Queries.

[31]—INTENSITY COILS.—Give the size and length of secondary wire, together with the method observed in the construction of the coils, then we may be able to assist you in determining the safe limit of battery power. If the insulation has been destroyed by "sparking," it will be necessary to unwind the wire until the faulty part has been discovered, mend the insulation with silk dipped in hot melted paraffin, and rewind the coil.—GEORGE EDWINSON.

[93]—WATER OF AYR STONE.—This stone may be cut with a disc, or a strip of sheet iron with sand and water, or it may be cut with a fine saw, as slate is cut.—GEORGE EDWINSON.

[119]—ELECTRO PLATING.—The six ounces of cyanide of copper can be worked out of the plating solution in the course of a few days by using a large anode of pure silver. Of course, the first deposits of silver will be inferior in colour, because alloyed; but I know of no other practical method. Separation by the chemical process would not pay for such a small quantity of solution.—GEORGE EDWINSON.

[152]—NICKEL PLATING.—To economise space here, I may mention that two good practical articles were given on this subject in the *Mechanical World* for Sept. 17 and Oct. 1, 1881.—GEORGE EDWINSON.

[228]—MICROPHONE.—Plates for this purpose, about 3 in. in diameter, the volume of the current increasing with size of plates. Any number until the required tension has been obtained.—GEORGE EDWINSON.

[229]—HAIR.—See, for accounts of almost instantaneous conversion of coloured hair to white, vol. i. pp. 198-99 of Hinton's "Physiology for Practical Use."—E. D. G.

[238]—ELECTRIC.—Inductive electric force is displayed in the space enclosed between the wires of a galvanic circuit, and also in the close vicinity of such wires. The "co-efficient of induction" would be the sum total displayed by two inductors, the amount of induction by one being known.—GEORGE EDWINSON.

[241]—FAURE ACCUMULATOR.—(1.) All the tongues of the lead-plates destined to form the positive element of the cell must be joined together. (2.) All the tongues of the opposite plates must be joined together, and "look" the other way. (3.) In series, one after the other, or side by side, as may be required. (4.) When all the oxide of lead has been converted into the peroxide of lead by the action of the charging current, the cell is fully charged, but the "quantity of electricity it will hold" varies with the age, &c., of the cell, for it improves with age. (5.) Open, if preferred, or protected from dust by a cover of wood. (6.) Yes. A cell might be charged with this force, or even less. (7.) No. (8.) The force of the battery does not depend upon the force of the charging battery itself, but upon the amount of chemical energy developed in the cells by the charge of electricity. Perhaps the discharging current does not realise more than 80 per cent. of the force expended in charging the battery.—GEORGE EDWINSON.

[242]—PROSE COMPOSITION.—The theory of English prose composition is admirably treated in Bain's "English Composition and Rhetoric," 1 vol. (Longmans); while original and useful practical methods are suggested in W. S. Dalgleish's "Introductory Textbook of English Composition," 1s.; or bound with the "Advanced Textbook," 2s. 6d. (Oliver & Boyd).—E. D. G.

[244]—LECTURES.—The "Working Men's Educational Union," some years ago, published an *Illustrated Reporter*, containing lists of subjects for lectures, and of diagrams published in connection with those subjects. The list before me comprises sets of diagrams in astronomy, geology, volcanoes, the steam-engine, the telescope, the microscope, the mechanical powers, and twenty-nine other subjects. These diagrams are now published, I believe, by the Religious Tract Society. Those which I have used were rough, but good.—E. D. G.

[247]—WARMTH AT NIGHT.—It is certainly conducive to health to maintain the body at a comfortable temperature at night; but have a care in the choice of stoves to keep the room warm. Unless the products of combustion, produced by a gas-stove, are carried out of the apartment by a flue, they will counteract all the beneficial effects of increased warmth.—GEORGE EDWINSON.

[263]—VEGETABLE FOOD.—If Provost P., or anyone else who desires any information about vegetarianism will write to Mr. Doremus, 30, Rochester-road, London, N.W., he will get information how to begin. A few stamps enclosed will further procure him some pamphlets.—T. R. ALLINSON, L.R.C.P.

[270]—BLOWPIPE CHEMISTRY.—"Amateur" seems to have puzzled himself sorely over Colonel Ross's very clear instructions for constructing his blowpipe. The piece of brass is to be soldered to the large end of the 12-in. telescopic tube, the mouthpiece being placed on the opposite end. "Amateur" will now see that there is no exit for the air blown into the tube through the mouthpiece. The jet (of which a drawing was given by Colonel Ross) is let into the telescopic tube at right-angles to it, and about half-an-inch from the stoppered end, fitting close on lines A and B of drawing. The hole in the jet, between lines A and B, now forms the exit for the air blown into the tube. The balloon is secured to the larger end of the jet, and acts as an air-chamber or reservoir, and, owing to its elasticity, a bellows. The brass nozzle is fitted to the small end of the jet, and thus forms the only outlet for the air blown into the tube. I hope this crude explanation will help "Amateur" over his difficulty. He seems to have mistaken the *jet*, of which a drawing was given, for the *tube*, of which only the dimensions were given.—PYROLOGIST.

[273]—STRENGTH OF MATERIAL.—There appear to me to be several errors in the solution given by "Anderson." First, he uses two different values for the distance between the guide-rail and the centre pivot—namely, 9·17 ft. and 8 ft.; and, again, he resolves an apparently vertical force into vertical and horizontal components. Taking 8 ft. for the distance between the guide-rail and centre pivot, and 25 ft. for the distance between the guide-rail and the direction of the weight, the solution is as follows:—

$$\frac{30 \times 25}{8} = 93\tfrac{3}{4} = \text{tension in tons on the centre pivot,}$$

$$\frac{30 \times 33}{8} = 123\tfrac{3}{4} = \text{pressure in tons on the guide rail,}$$

$$123\tfrac{3}{4} - 93\tfrac{3}{4} = 30 = \text{weight in tons on the crane.}$$

—F. M.

[274]—FORAMINIFERA IN CHALK.—Brush out with a hard brush, well wash in water, and pick out with a needle, and mount in balsam.—ALBERT SMITH.

[274]—MISCELLANEOUS.—(3.) The following drawing-books may be recommended to "Eozoon":—1. "Vere Foster's," Marcus Ward & Co.; 2. Cassell's series of "Popular Drawing-Books"; and 3,

those published by Blackie & Sons, under the direction of E. J. Poynter, R.A.—Robert Macpherson.

[274]—Miscellaneous.—The treatment of chalk foraminifera differs somewhat from that of foraminiferous sand. A very satisfactory method is to boil a small piece of chalk with caustic potash in a test tube until it separates in fine powder. This is shaken up in a large bottle full of water, the foraminifera are then separated by specific gravity, the water being poured off after standing a short time, and a fresh supply added as long as it comes away of a milky tint. The deposit will consist chiefly of foraminfera, and may be mounted in balsam. I have prepared several slides of chalk from the North Downs, in Kent, by this method, which I have always found to answer perfectly well.—C. Harris.

[275]—Salt.—The objection to the use of salt can only be accounted for by some peculiarity of taste. It is certainly not shared, as far as I know, by any of our "high medical authorities." By the use of salt the salivary glands, the secretion from which aids in the digestion of food, are stimulated. Its use is, of course, not absolutely necessary; but that it is the most natural of all condiments, and therefore the best, is made evident by the fact that saline matters are contained in all kinds of natural food.—Robert Macpherson.

[275]—Salt.—Many object to salt on the idea that it is an acquired taste; they say salt is not a natural part of man's food, and so we ought not to take it. They instance children, who always splutter out salt food when given them. It creates a false appetite, and a craving for food even when the stomach is full. Also giving rise to thirst, or a desire to drink. It increases the flow of gastric juice and of saliva, for a time, the same as any other bitter substance will. Some object to it on the ground of it being a mineral, and say that our food contains all that is needed without it. I know many vegetists who never take it from year to year, who cook everything without it, and who even have it not in their houses.—T. R. Allinson, L.R.C.P.

[277]—Luminous Paint.—This consists of calcium sulphide, ground in oil. The light given by it is bluish. A 6-inch square surface will show time by watch. Of course, the paint must have been recently exposed to light.—Lewis Arundel.

[277]—Luminous Paint.—This is sulphite of calcium, made by burning oyster shells in a closed vessel with sulphur.—Albert Smith.

[278]—Smell from Burning Gas.—The gas is imperfectly consumed. The globes sometimes cause a rush of air, through the hole being too small, and then the gas is not consumed.—A. Smith.

[282]—Smelling Salts.—The use of smelling salts is only beneficial in cases of fainting and nervous depression, and at times in neuralgic headache. Women use them more frequently than men for the same reason that men use tobacco more frequently than women, the force of custom in both cases being the ruling power.—Robert Macpherson.

[284]—Pencil-point Protectors.—You can re-silver, using cyanide of silver: it is very poisonous.—A. Smith.

[285]—Scientific Terms.—Collins & Sons publish an illustrated Dictionary of Scientific Terms, by W. Rossiter (price 3s. 6d.); very useful and compact. Anything not in Rossiter is easily found by taking the Roots and consulting a Latin or Greek dictionary, as the case may be.—G. B.

[286]—Electricity.—Get Sprague's "Electricity," published by Spon, Charing Cross.—A. Smith.

[297]—Tobacco and Science.—Tyndall has shown that the blue of the sky and the blue of the sea are caused by the breaking up of the rays of light by infinitely small particles of solid matter. The blue smoke rising from the glowing end of a cigar or pipe contains very minute particles of carbon at a high temperature. After the smoke has been drawn into the mouth and expired, two changes have taken place—the smoke is at a lower temperature and laden with moisture. It is also heavier. Perhaps there has been some chemical change in addition. At any rate, we may safely say the atoms of carbon have united into larger particles—just as coal-smoke particles form smuts—and reflect light in a different manner. The smoke issuing from the paper tube of a cigarette contains the two smokes—a small quantity of smoke at a high temperature drawn back from the glowing end, and a larger quantity of cold smoke that has been in the smoker's mouth, which has been rendered darker and heavier.—W. D. B.

Photographic Bromide Solution.—Ammonia bromide, 60 grains; liquid ammonia, 4 drachms; dis. water, 12 drachms.—A. Smith.

[Letter 309]—Duration of Life.—S. wants to know what I mean by stating that it depends on ourselves whether we die at 35 or at 70. It is this, "All diseases are due to three conditions":—1st, my parent's fault; 2nd, my neighbour's fault; 3rd, my own fault. 1st. If my parents transmit to me a tendency to gout, consumption, or insanity, then the fault of ill-health belongs to them. If a person dies before 20, it is the fault of his parents, after that it is his own fault. 2nd. If my neighbour's drain runs over, poisons my drinking water and I get typhoid fever, then he is to blame. If another one comes to my house whilst he is suffering from small-pox and gives it to me, then surely I am not to blame. 3rd. This is the largest class, and contains most diseases, for by care the tendency to gout, &c., can be rooted out from the system. Given many persons taking rich food and much of it, then bilious attacks are to be looked for. If the rich food be continued for a long time, then expect gall stones and stones in the bladder. Give alcohol, and we get chronic indigestion, rheumatism, &c. Give rich food and alcohol, and we get gout, apoplexy, heart, kidney, and liver disease. Give tobacco, expect sore throat, palpitation, slowing of heart's action, and in some few cancer of the lip. If fresh air be neglected, then expect colds on the least exposure. If exercise be not taken, we expect constipation, piles, congestion of liver, &c. Now add up the action of lots of meat, plenty of drink and tobacco, and we shall get our people dying at 35 years. Take all in moderation, and an average constitution will last till nearly 70. But by abstaining from beer and tobacco and being spare with meat, by taking plenty of exercise and fresh air and keeping the mind calm, we may go to fourscore or more.—T. R. Allinson, L.R.C.P.

Answers to Correspondents.

*** All communications for the Editor requiring early attention should reach the Office on or before the Saturday preceding the current issue of* Knowledge, *the increasing circulation of which compels us to go to press early in the week.*

Hints to Correspondents.—1. *No questions asking for scientific information can be answered through the post.* 2. *Letters sent to the Editor for correspondents cannot be forwarded; nor can the names or addresses of correspondents be given in answer to private inquiries.* 3. *No queries or replies savouring of the nature of advertisements can be inserted.* 4. *Letters, queries, and replies are inserted, unless contrary to Rule* 3, *free of charge.* 5. *Correspondents should write on one side only of the paper, and put drawings on a separate leaf.* 6. *Each letter, query, or reply should have a title, and in replying to letters or queries, reference should be made to the number of letter or query, the page on which it appears, and its title.*

A Reader and Subscriber to Knowledge. The subject is fully considered in treatises on physiology.—Mathematicus. Mathematical course at Dublin is excellent.—J. M. You are not content with 2¼d. for 2d., but insist on 3d. or 2¾d. at the least. The paper is excellent.—H. A. Bulley. You evidently misapprehend what science says on the subject. You carefully make a straw giant, and as carefully upset him.—Tyro. As to your first, ask us an easier one. As to the other, to find how much a ton at earth's surface is attracted to the moon, multiply a ton by the square of 3,960, divide by square of 238,818, then by 81, and you will get the mass whose downward pressure at earth's surface is equal to attraction of a ton of matter (on earth) moonwards.—Nicholas Morgan. Pardon me, the phrenologist contends for something more than that difference of cranial form indicates a like difference of disposition, or of talent, or of form. *So much*, many admit who are not phrenologists. The Mallicolese skull compression cannot, however, afford much ground for argument either way.—Teeld, or Jecla, or Yeela (??). Have never heard of Dr. Long's "Astronomy." For a beginner, should say Guillemin's "The Heavens" (Bentley) would suit.—A. N. That frog cure is really too absurd, also too cruel.—Celt wants his theory discussed, that the builders of the Great Pyramid heaped up earth all round the Pyramid so as to form a slanting surface to the Pyramid level, as it rose layer by layer, and afterwards removed all that earth. He thinks—but does not say why—that this would explain the slant passages. He asks if any one can offer a better theory. "In those days," he says, "engineering appliances were not likely to be able otherwise to raise such great blocks of stone, and labour was practically unlimited." It must have been, if the theory is correct.—Charles Horner. It would be simply absurd to tell our readers that the Great Gallery has been measured to within a hundredth of an inch. No one who knows anything about measurement would believe it of any gallery fifty yards or so long, still less of the Pyramid Gallery. You could make the length anything between 1,870 and 1,890 in. according to the way you chose to measure, and anything between 1,800 and 1,950, according to the length you choose to assign to the pyramid inch. These coincidences are mere tricks, though honestly meant enough, I have no doubt. The length of York Minster is quite as near the 1,000 millionth of the sun's mean distance as the Pyramid's height; so is the height of Rouen Cathedral; but no one has yet started any craze about the divine

inspiration of the architects of either cathedral.—W. A. C. It is admitted, by so staunch an opponent of cruelty to animals as Mr. R. H. Hutton, that if all men became vegetarians, multitudes of animals now used for food would of necessity perish for want of food. This being so, it is not fair to tell the creophagist that though "he can get all the chemical qualities of meat without taking animal life, he takes it in order to tickle his palate." Note also that F.R.A.S. only speaks for himself; he does not attack others; you go out of your way to charge him with cruelty. Observe, I am quite with Mrs. Dr. Kingsford and other vegetarians in all that they say about the coarse and disgusting scenes now associated with the supply of flesh meat. But I think the uncompromising vegetarian does more to prevent change than to hasten it. The world at large would be easily moved, I think, to see that our meat supply was obtained in a better fashion; but you do not aim at that—you try to persuade the world to admit at once that animal food should be dispensed with; and the world replies in effect, If *that* is what you want, you are not likely to get it.—TARANAKI. If you read Sir E. Beckett's book more carefully, you will find that where he writes, "Chance is only the uncalculated result of some known or unknown laws of nature" (these are his words, but your quotation is near enough), he means only what he expresses thus later on, "The only meaning of the word 'chance' in the physical universe, since it began to exist, is this sort of incalculable consequence either of the known or unknown laws of Nature." In other words, he is referring to the word "chance" as applied to natural processes. What I have spoken of as pure chance he speaks of as "bare chance" at p. 19; and, of course, like me, he not only "believes," but knows "there is such a thing." To use his own illustration, "every rational man concludes" that the atoms of the universe have been, "as we say, of dice, 'loaded,' to make them behave in a particular way, not a thousand or a million times, but always," . . . "because the only possible alternative is that of bare chance; and the idea of all the atoms of the universe behaving as they do by chance, is too absurd for any man in his senses deliberately to entertain."—TANGENTIAL TENDENCY. No; it cannot be said that because the sun's attraction acts in same direction at one part of the earth's orbit as the tangential tendency acts a quarter of a revolution later, the sun's attraction during one quarter causes the tangential tendency in the next. The direction of the earth's motion is changed, but the earth's velocity in the tangential direction is not generated by the sun's action.—A READER OF KNOWLEDGE. The difference is due to use of mean instead of solar time. We shall treat fully of this presently; it cannot readily be explained in few words. Name a good science manual? Would you mind mentioning on what subject?—HENRY BOWMAN. We should be glad to give more chess, if we could; but others want less, and many want more mathematics.—EDWARD HARGREAVE. We should be glad to give more whist, if we could; but others want less, and several want more chess.—H. B. R., CHARLES JERROLD, M. JAMES, and others. We should be glad to give more mathematics, but others want less, and many ask for more whist.—H. W. FAWCETT. Two of my essays on the Pyramid are in my "Myths and Marvels of Astronomy," two in my latest work, "Familiar Science Studies," both published by Messrs. Chatto & Windus.—W. MCMANUS. Your query would lead to endless replies. The opponents of evolution are many, their arguments numerous. Read, as one of the best examples, Sir E. Beckett's little book on the Origin of the Laws of Nature (S.P.C.K.), also Dawson's Story of the Earth and Man (Hodder & Stoughton)—a really charming work.—SIMPLEX. I am very much obliged to you for so carefully, in response to my wish, showing some examples of Bell's line-writing; but although, from a scientific point of view, it may (I cannot honestly say I see why) be beyond comparison with anything preceding it, it seems to me to compare unfavourably with Pitman's in brevity. The double curves for *v, f, l, r, w* (English) seem serious objections from a stenographic point of view.—PARADOX. He was kind enough to send me his confounding of Darwin.—JOHN J. PRINCE. Considering that the subject of the great changes of climate which different parts of the earth appear to have undergone, occupies many hundreds of pages in divers treatises on geology, you should hardly expect us to put the matter as a query, inviting readers not only to lucubrate thereon, and to give reasons for their opinion, but if possible to demonstrate it mathematically. The subject is one in which we may shortly offer an article by an acknowledged master of the subject, but for correspondence and replies,—not much: (for we should get too much). As to the other question (which you ask over name TYRO), whether the earth's diameter is being increased by layers formed out of its own substance, the question seems akin to this, When Pat took a strip a foot wide from the bottom of his blanket and added it to the top, how much did he increase the length thereof?—W. C. You are angry because we will not give up the fine saying by Liebig. Well, you make a mistake in this; and, as Liebig says, there is no harm in making a mistake. But before "putting us down," do consider the harm you propose to do us.—W. H. H. SOAMES. Thanks for your courteous letter. May I, for a last word, say that perhaps if those who have devoted much time to the account in question were at one in their interpretation, Science might do well to consider it more attentively than at present. What say you to this from Monsignor Clifford, respecting the account?—"C'est nullement l'histoire de la création, fait en sept jours ou sept périodes de temps, mais simplement la consécration, sous forme d'hymne sacré, des sept jours de la semaine à la mémoire, au souvenir, des sept œuvres principales de la création." Whereunto my excellent friend, M. l'Abbé Moigno, replies:—"Oserai-je exprimer le regret que Monsignor Clifford ne partage pas mes convictions de la vérité de la cosmogonie de Moïse, quoique mystérieuse encore dans quelques-uns de ses détails."—HISTORIAN. Queries answered, I think, in this section; history scarcely falls into our line.—C. T. B. Surely the description of tobacco smoke as "a cloud of tar in half-burnt gas" is inexact.—J. H. WARD. *Erravi*; you can either correct as you suggest, or for mean distance write "perihelion."—A. T. C. Newton settles it, does he not? yet he says *Hypotheses non fingo.* Doubtless he came near the truth; but it was not a matter of observation, experiment, or mathematical demonstration. Wonder who told him.—AYRSHIRE wants book on Ventriloquism, with dialogues.—PRIVATE STUDENT. Such questions are not suitable; we can neither insert in Mathematical Column nor answer here. We should be flooded with such questions if we did.—C. C. C. You seem to think I keep all the questions and answers in my head. I cannot tell what theory you refer to unless you give page and column of my answer.—IGNORANT. Any text-book of biology will tell you how fish breathe by means of their gills; the air in the water which passes through the gills being "what they breathe."—J. P. SANDLANDS. We have treated you with courtesy and consideration; you repay us by accusing us of unfairness and cowardice, because we decline to break through a rule which we consider absolutely essential to the maintenance of KNOWLEDGE in its proper position. We shall say no more.—EDGAR FLOWER. George Stephenson, the engineer, was, of course, right in saying that the uppermost point of a wheel of a carriage moves twice as fast as the carriage, while the lowermost is for the moment at rest: does this require elucidation?—T. W. JOHNSON. If you are "thoroughly satisfied" it is a delusion, all is well so far as you are concerned.—J. MCGRIGOR ALLAN. We are quite with you, but have not room for the subject, which does not belong to our programme.—A. DANIELS. We cannot find time or space to work "sums."—JNO. TRIST. We regret that the necessities of space will not permit us to find room for "a few essays on the doctrine of philosophical necessity."—H. H. L. HILL notes that the collection of the late Professor Tennant are still on sale at a shop a few doors west of King's College, and will shortly be sold by auction. Suggestion noted as to centre of pressure, &c.—C. HARRIS. The attraction of the mouthpiece is virtually *nil*. As to the colour, there can be little doubt you are right, and that the smoke looks blue only when seen by reflected, brown only when seen by transmitted light.—PROFESSOR BUCHHEIM. Your letter appears.—JEMIMA. Scarcely space at present for the refining of sugar.—E. F. Scarcely a reason,—rather How, than Why. —JOHN SPARKS writes "i" for "I," except when he forgets his part. He may note with advantage that the word "science" would not be spelled "cience" by one who wrote "i" for "I."—F. BLAKER. The law of diffusion of gases would not affect, to any appreciable degree, the *stagnation* of the air in upper part of room, and would only very slowly cause the carbonic acid gas continually poured into that part of the room (by persons breathing) to be diffused; in fact, it would act much more slowly to diminish the carbonic acid gas than the breathing even of one person only would tend to increase it.—CLARE. You are rather hard on Mr. Abbott; we do not think he wants to study a treatise of either class, but simply to know what is commonly understood by the expression "abstract reasoning." The reply about tobacco smoke assumes more than can very easily be proved. Iron certainly does rust faster in salt water.—W. WILSON. I agree with you that there are *cases* where ordinary modes of expression are misleading, "A tog is a tog, but the question is, is *this* tog a tog?" for which you'll overhaul your "Snarley Yow," and when found make a note of.—E. D. G. There are certainly cases in which the differential equations admit of more than one solution; but it has not been shown that such cases can occur in nature. Till this has been shown M. Paul Janet's inference is but a rather fanciful guess. The question is too difficult and artificial to much interest the bulk of our readers.—B. RILEY. Question already answered satisfactorily.—L. D. S. You should get an elementary book on astronomy.—E. W. C. The one with larger capital, whether A or B, has the best chance of winning in the long run. See my essay on a Gambling Superstition.—ERIN. As you think the outside car is the best ever invented, you ought

not to ask why I think it appropriate to Irish girls. Moon's phases can scarcely be explained here, as they are explained in every respectable text-book on astronomy.—H. D. Barclay. A man *might* be said to be evolved from a baby, or an oak from an acorn; but in the science of our time the term evolution is generally applied to the development of species.—A Schoolboy. The reason is that the earth goes round the sun once a year, so that the stars on the side opposite the sun necessarily change. Imagine a long, straight line from the sun to the earth and beyond. This would point in one direction at the beginning of a month, in a changed direction (because of the earth's motion) at the end.—W. B. Grove. Pardon me, it is you who are wrong in denying that to give an equal chance of a particular event, such as all the trumps being in one hand, there must be a certain number of trials. You are right in saying one particular arrangement is as likely as any other, but it is very far, indeed, from an even chance that that particular arrangement will appear. You shall correct the mathematical discussion of this point, if you like and can, when it appears.—Archimillion. Your "cosmetical specimens of angularised forms" too much for us: we need rest after your letter.—W. W. There is nothing to make the earth assume a more upright position. The removal or burning of all the coals in the globe could not affect her axial pose, even if they were all at one spot in the northern hemisphere.—S. S. H. By triangle of forces, the actual wind FE (Fig. 2, p. 36), and the wind EC, resulting from yacht's motion in direction CE, would be balanced by a wind CF, and, therefore, their resultant is a wind FC. 2. Well, you might, for such a problem as you require, take this, Assuming frictional resistance constant, and sail GC always set to bisect the angle FCE (as this angle varies with the increasing velocity of the yacht), determine the maximum velocity which the yacht can attain on a given course and with a given wind. Do not ask me to solve this, please, for time will not permit.—A. H. Moseley. We partly think with you that the stories on "Intelligence in Animals" have now run far enough; but we have to consider, what perhaps you overlook, that many readers take much more interest in such matters than in those with which you wish to see Knowledge filled. Your friend who told you that the very amusing and well-written sketch you object to would have been sent to the W.P.B., but for the military rank of the writer, is no doubt one who understands very thoroughly his own nature, and judges others accordingly. Those who know me best tell me I err in the other direction. Need I say I do not agree with them? but at any rate, they know me; your friend very manifestly does not. You say "give us more astronomy, instead" of such articles; but for one who so addresses me there are tens, or rather fifties, who say give us more that is light and readable. Of course, if all readers wanted deep scientific matter, I should feel bound to consider them; but I expect that would mean such a limited array of readers that Knowledge would very soon come to an end. This point has to be considered, you know. Oddly enough, you are the first who has written objecting to the introduction of these stories (except one who gave a semi-religious reason). *Now* the round is complete, and *every* subject dealt with in Knowledge has had its turn. If all were listened to, therefore, we should appear with blank pages. Yet all who have criticised special subjects have done their best to advance our success. Many thanks for what you have done in that way.—Geo. Robson. We are thinking of having "vellum wove," with illuminated borders and headings. Your own special copy shall be printed in gold letters on purple-tinted silk, delicately scented. Yet we fear you would not even then be happy. Will it cheer you to be told that the average cost of the numbers since January 1, instead of being less, as you fondly imagine, has been half as great again as that of the first nine numbers? In this, of course, is included one very important item, which persons who, like yourselves, complain of paper, folding, and so forth, persistently overlook.—C. O. Thanks for your very pleasant letter; it does me good after G. R.'s sour one. You have not the problem at p. 380 quite right; you take a square *plus* a square, and the figure is a square *minus* a square.—Gnats. Very likely you are right in assuming that thin-skinned persons are preferred by biting insects to those with less penetrable cuticles. — Lewis Arundel. Questions already answered, or in course of being so. The mistake in letter 289 is annoying; it is one of those cases of perverted ingenuity which are apt to escape the "reader," for his attention is directed to orthography, punctuation, syntax, &c., so that the absurdity of a statement, as such, is apt to escape his notice.—Percy B. Dodd. Thanks; but the problem has been already dealt with in Knowledge, No. 8, p. 166.—Edward Stubbs. I have carefully compared the weather records for the years 1768 to 1792, left by White of Selborne, and find not the slightest balance (even) of evidence in favour of the theory that the weather repeats itself after seventy-six years, in England, at any rate.—F. Cowley. Thanks. About photography, Mr. Brothers, of Manchester, has kindly promised to write for us. The other questions already answered. — Hallyards. I did not mean that I could just see the *lumière cendrée* in America a day before first quarter, but that it was then strikingly obvious—as obvious as in England when moon shows but a narrow crescent. When you saw objects more clearly through moist air, they must, you think, have been magnified; but were they? Surely you could tell whether objects looked larger or not. It is absolutely impossible that moisture in the air should magnify. I am quite with you about differences of eyesight. I can see details with my naked eye (literally eye, for it is only one which has the power) which some find difficult to see with an ordinary magnifying glass. But personal differences such as that have no bearing on the question whether moist air magnifies. You speak of a change of tone in my communications to the E. M. after 1872; possibly you refer to letters in which I exposed the jobbing schemers. Truly, then I did *not* mean "legitimate playfulness." I laid on the lash with intent, and with tolerable effect. Looking back now at that time, I see nothing to regret except occasional leniency. Sorry it jars on you to hear our writers speak of "Huxley" and "Darwin" and so forth. It would jar very much on me to speak of Mr. Darwin or Mr. Herbert Spencer. Will ask the publishers whether the quadrifid ornaments on either side of the heading of each page ought to hurt readers' eyes. Are you not rather sensitive? The large maps can be bound up as the double-page pictures in the *Illustrated* and *Graphic* are bound. As to the words which seem to imply that the Egyptians were acquainted with the earth's motion, surely there is some difference between them and references to the rising, setting, and motion generally of the sun and moon. These bodies seem to move—the earth does not. *Does* an open fireplace help to ventilate a sick room? I know something on that score from experience, and I venture to say that if there *is* a place where our open fires are greater nuisances than elsewhere, the sick room is the place.—Jno. Sutherland. We cannot undertake the office of private tutors to individual readers. Nine-tenths of the initials, &c., under head "Letters Received" belong to correspondents who ask such question as yours. If we had space, we should not have time. Again, if you must ask questions involving the use of the differential calculus, why use the fluxional notation? To use Babbage's old joke, we prefer d-ism to dot-age. Lastly, to your question: (we answer just this once) you could hardly expect to get the right differential equation if you treat Υ as a constant, even though it be only once. You have $\frac{d^2y}{dt^2}=\Upsilon$, and you say $\therefore\left(\frac{dy}{dt}\right)^2=2y\Upsilon+C$; but Υ is not constant; indeed you write down eventually to an equation in which there is a term involving $\frac{d\Upsilon}{dx}$. "That's how the error has arisen."—R. C. Fraser. We must wait till the microscope has shown the diamonds in the razor's edge before we take that for a reason.—A. Blake. Other suns and systems might, of course, have originated as our sun and his system have done. We are not nearly so clear how this has been as you seem to imagine. The "sweet influences" of the Pleiades have been explained in several ways; there can be very little doubt the reference is to the supposed influences of the Pleiades when the sun was in their neighbourhood; just as Sirius was supposed to combine his rays with the solar rays, later on in the year, to produce unpleasant effects, so in early spring (when in Job's time the sun was passing the Pleiades) the germinal influences of the season were attributed to those stars.—J. M. Fothergill. Of course, Cotton Mather was an American; I thought everyone knew that. The story is told by Wendell Holmes, in his Breakfast series. I referred only to the "irreverent." It struck me as good fun to hear that strict, solemn, and most rigid Calvinist charged with irreverence—enough to make him turn in his grave; albeit one may note a good deal that is very irreverent indeed, in my opinion, in his "Remarkable Providences."

Letters Received.

A. Hargreave, J. J. Snellgrove, M. M., Neminis Umbra (we cannot see it), Willow, M. Paterson, J. K. L., M. Petersen, Blue Peter, An Admirer, Constant and Thorough Reader, Lucasian, P. Parley, Titmouse, Aggrieved One, Patience (just what Aggrieved One wants), L. Prowse, Ternary, S. Smithson, B. J. Harvey, Amused, Vegetarian, Prospero, M. Macnulty, Jerry, F. T. Heffernan, Harpax, Morose, Mother Shipton.

Pond's Extract is a certain cure for Rheumatism and Gout.
Pond's Extract is a certain cure for Hæmorrhoids.
Pond's Extract is a certain cure for Neuralgic pains.
Pond's Extract will heal Burns and Wounds.
Pond's Extract will cure Sprains and Bruises.
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Notes on Art and Science.

HAIR TURNING WHITE.—A person I know of (brother-in-law to an old friend of mine) met with a railway accident, which turned his hair perfectly white. I do not know if instantaneously. He lived to be an old man, and after death his hair turned, or rather returned, to its original colour, dark brown.—JOHN ALEX. OLLARD.

EXPERIMENTS have repeatedly been made with the object of producing natural imitations of the craters and inequalities visible on the moon's surface, and it has been found that the figures of the lunar inequalities can be closely imitated by throwing pebbles upon the surface of some smooth plastic mass, such as mud or mortar. Mr. Meydenbauer, of Marburg, uses a basis of dextrine for the purpose, and drops small quantities of the same material from a moderate height upon that basis. A photograph of various figures which are thus produced, shows a remarkable resemblance to the various inequalities visible on the moon's surface.—A. MARTH, in the *Academy*.

SOOT V. POLLEN.—I have often noticed that although the hazel will grow and flower freely in the suburbs of London, yet it will rarely produce fruit. To gather nuts you must go some miles into the country. A few days ago, I was examining the catkins and female flowers of the hazel (gathered about four miles from the city) under the microscope, and I was struck by the fact that the pistils were each severally coated with a deposit of soot, sufficiently thick to prevent any chance of fertilisation. The hazel being anemophilous, the absence of nuts in the neighbourhood of London (and, I presume, of other large towns) is thus, I think, sufficiently accounted for.—WILLIAM H. ALLEN.

MICROCOCCI IN MUMPS.—The *Gazette Médicale* says that MM. Cabitan and Charrin, at a recent meeting of the Biological Society of Paris, gave an account of the investigations which they have for some time been engaged in, on the presence of minute organisms in the blood of persons suffering from mumps. These are multipliable by cultivation in Liebig's broth, and are found to consist of minute *batonnets*, but chiefly of micrococci, all in a state of motion. These minute organisms, they consider, corroborate the clinical observations which tend to place mumps among the infectious diseases. The absolute proof that this disease is due to these minute existences, by reproducing it by inoculation of the "cultures," has not been attained by the experiments made to that end.

ENGLISH AS THE SPEECH OF THE FUTURE.—The success of the English-speaking peoples as colonists, and their superior prolificness, are not the only reasons for thinking that the English tongue is destined to dominate the world. The flexibility and terseness of the English language has made it the language of international telegraphy, and from statistics just collected it appears to be the great newspaper language. In other words, it about equally divides the newspapers of the world with all other tongues combined. The total number of newspapers and periodicals now published is given in H. P. Hubbard's forthcoming "Newspaper and Bank Directory of the World," as 34,274, with a circulation of about 116,000,000 copies, the annual aggregate circulation reaching, in round numbers, 10,600,000,000 copies. Europe leads with 19,557, and North America follows with 12,400, the two together making over nine-tenths of all the publications in existence. Asia has 775; South America, 699; Australasia, 661; and Africa, 132. Of all these, 16,500 are printed in the English language, 7,800 in German, 3,850 in French, and over 1,600 in Spanish. There are 4,020 daily newspapers, 18,274 tri-weeklies and weeklies, and 8,508 issued less frequently. It appears that while the annual aggregate circulation of publications in the United States is 2,600,000,000, that of Great Britain and Ireland is 2,260,000,000.—*Scientific American*.

THE SOUND OR SWIM BLADDER OF FISH.—Perhaps the following quotation from the "Icelandic-English Dictionary," by Cleasby and Vigfusson will be sufficient to satisfy your readers as to the derivation of the word "sound" or "sounds" as expressive of the air or swim-bladder of certain fishes. The word is spelt "sund." "Sund (*q.s.* svund), [from 'svimma,' dropping the *v* and changing *m* into *n*]: 'a swimming';" &c. Some compound words are given as "sund-fjöör" (*f.* "a swimming feather"); "sund-fœrr" (*adj.* "a good swimmer"); "sund-hreifi" ("a swimming pair"), of a seal with several others. It is said that "swimming was a favourite sport, the antagonists trying to duck one another," and that "sund" is one of the sports in King Harold's verses. It is also added that the word "sund," as used to denote a "sound" "or straight, narrow passage," is quite a different word from the preceding, being derived from "sundr," *i.e.*, "that which sunders." So that the proposed connection of fish "sounds" with words (having a somewhat similar appearance) in the Sanskrit, Assyrian, Chinese, Egyptian languages, &c., denoting "blood," "heart," &c., has no real existence whatever. The Scandinavians were doubtless aware of the part which "the sound" plays as an aid to a fish's powers of swimming, and never attributed to it the function of an aorta. They knew better.—W. HOUGHTON.

ANIMAL VACCINATION.—What Pasteur calls the "vaccination" for the "anthrax" disease has been shown by repeated experiments to be absolutely protective. Professor Greenfield has vaccinated cattle from rodents (gnawing animals like rats, squirrels, &c.) with the "anthrax disease," and has found that they remain free from all disorder, local or constitutional. The same result has attended M. Toussaint's experiments with the bacillus "cultivated" in special fluids, not in the living body of any creature; sheep and dogs inoculated with this cultivated poison showing no form of the deadly "anthrax" disease. The experiment was conducted on a large scale under the auspices of the provincial agricultural societies of France. A flock of fifty sheep was placed at M. Pasteur's disposal. Of these he vaccinated twenty-five with the cultivated "anthrax" poison on May 3, 1881, repeating the operation a fortnight later. All the animals thus treated passed through a slight illness, but at the end of the month were as well as their fellows, the twenty-five which had not been vaccinated. On May 31, all the fifty were inoculated with the strongest anthrax poison. "M. Pasteur predicted that on the following day the twenty-five which were inoculated for the first time would all be dead, whilst those protected by previous 'vaccination' with the mild virus would be perfectly free from even mild indisposition. A large assemblage of agricultural authorities, cavalry officers, and veterinary surgeons met on the field the next afternoon to learn the result. At two o'clock twenty-three of the unprotected sheep were dead; the twenty-fourth died an hour later, and the twenty-five at four. But the twenty-five 'vaccinated' sheep were all in perfectly good condition; one of them, which had been designedly inoculated with an extra dose of the poison, having been slightly indisposed for a few hours, but having then recovered."—R. A. P., in the CORNHILL MAGAZINE.

MR. RUSKIN ON EDUCATION.—Mr. E. J. Baillie, of the Ruskin Society, is contributing a series of articles on Mr. Ruskin and his Teachings to House and Home. In the article on "Education," Mr. Baillie says: "In one of his books Mr. Ruskin has spoken pointedly upon the prominence and precedence almost invariably given to what may be termed caste, or class distinction. He has told us that there is a widely expressed desire for 'an education which shall keep a good coat on the back; which shall enable [a son] to ring with confidence the visitors' bell at double-belled doors; which shall result ultimately in the establishment of a double door to his own house; in a word, which shall lead to advancement in life; *this* we pray for on bent knees; this is *all* we pray for . . . It never seems to occur to the parents that there may be an education which in itself is advancement in life. That any other than that may perhaps be advancement in death. . . . To many "advancement in life" means . . . in a word, the gratification of our thirst for applause. That thirst, if the last infirmity of noble minds, is also the first infirmity of weak ones, and, on the whole, the strongest impulsive influence of average humanity; the greatest efforts of the race have always been traceable to the love of praise, as its greatest catastrophes to the love of pleasure.' In the scheme of national education, Mr. Ruskin realises the necessity for national Government schools. He maintains 'there should be training-schools for youth established, at Government cost, and under Government discipline, over the whole country; that every child born in the country should, at the parents' wish, be permitted (and, in certain cases, be under penalty required) to pass through them; and that, in these schools, the child should (with other minor pieces of knowledge hereafter to be considered) imperatively be taught, with the best skill of teaching that the country could produce, the following three things:—(*a*) The laws of health, and the exercises enjoined by them; (*b*) Habits of gentleness and justice; and (*c*) The calling by which he is to live.'"

Our Mathematical Column.

THE LAWS OF PROBABILITY.

BY THE EDITOR.

THE general law enunciated in our last number may be regarded as the fundamental law of probabilities. Nearly all problems in probabilities, direct or inverse, depend on this law, to which the more complex cases are reduced by various devices of greater or less simplicity according to the nature of the problem. And again, the value of any chance not relating to tickets in a lottery, or balls in a bag, may readily be

referred to these easily-understood illustrations. Thus (to take in passing a very familiar case), suppose we are told that the betting is four to one against a certain horse in a race, what are we to infer is the value of the chance which betting experts assign to him. The odds of four to one mean, of course, that there are four chances against the horse for one in his favour, or that out of five equal chances he has one. His chance of winning is, therefore, *so far as the betting shows*, equal to that of drawing one particular ticket from a bag containing five, all equally likely, antecedently, to be drawn. I need hardly say that the real chance of the horse may be very different. The betting experts may not know nearly so much as they suppose, and the horse may have a much better or a much worse chance than they imagine. Or again, they may know (some of them) much more than they pretend to know. But so far as the betting shows, that is the horse's chance. We shall see further on how this simplification of the problem enables us to determine from the known betting about two horses singly, the proper betting about the pair, and similarly for three or more horses.

Again, take cases which at first sight do not seem to resemble the problem we have just considered—as, for instance, the tossing of a coin or the casting of a die more than once—and let us see how these can be reduced to the simpler case. Here is a simple problem of this kind:—

What is the chance that, when a coin is tossed twice, the tossings will be unlike—that is, that both will not be heads nor both tails? Simple as this problem is, by the way, the great mathematician d'Alembert went astray in dealing with it (at a time when the mathematics of probabilities were not very well understood). He reduced it to our general law in summary but inexact fashion. Thus he said, There are three possible events: either both tossings will be heads, or both tails, or they will be unlike; therefore the chance that they will be unlike is one in three, or one-third. This result d'Alembert maintained with a degree of confidence which seems singular when the simple nature of his mistake is considered. To solve the problem correctly, we must proceed, as he did, to consider the various possible events; but not err as he did, by failing to notice that he counted two of them as one. The possible events are four, viz., head, head; tail, tail; head, tail; and tail, head. There are manifestly no others, and as manifestly any one of these is as likely as any other. Now of these four cases, two give unlike tossings, viz., the two last. The real chance, therefore, is not one in three, but two in four, or one half.

If d'Alembert had been a betting man, and had backed his opinion by wagering two to one against the occurrence of unlike headings, during a great number of trials, he would have lost heavily—the real wager being even. In the long run, half the trials would be of the kind against which he had wagered; and losing thus as often as he won, while paying twice as much when he lost as he received when he won, he would manifestly lose just as much as his opponent had wagered. At least, the result would approach this (and his loss, therefore, be the greater) the more numerous the trials.

We note, then, in passing, that men who are unwise enough to gamble ought to have a clear idea of the laws of chance; for in the long run these are as inexorable as the laws of the Medes and Persians. A man may buy a single ticket in a lottery, and he may chance to win, or he may try his luck pretty often at *roulette* or *rouge et noir*, and still be a winner; but if he keeps on long enough, he will inevitably lose in proportion to the calculated chances against him. (He may lose at the outset, and probably, on the whole, it is the better for him that he should.)

There are, indeed, ways in which men suppose that with patience they can always win, though slowly. The following is a familiar illustration, which we leave as an exercise for the reader:—

Two gamblers, A and B, toss a coin on the following terms. A wagers against B evenly £1; if B wins A wagers £2 even with him; if B wins again A wagers £4 even with him; and so on, doubling each time, till A wins the toss. When this happens, whatever the number of tossings before the event, A wins £1. They begin again, A wagering £1 even, as before, and doubling till he wins, when he pockets another £1. Every wager is strictly fair, yet every trial (as A must, at last, win) ends by A winning £1. The system seems altogether unfair in its results, though perfectly fair in details. Is it so or not? Assume A and B to have equal capitals, say £1,000, and estimate their chances of success or failure. At first sight it seems as though A must gradually win every pound B has. In reality it is not so, as we shall show later.

"All Roots."—(See "Answers to Correspondents," p. 391, line 17.) In reply to "All Roots'" query, I give an easy method of extracting the fifth root of 5153632, and will endeavour to state the rule as briefly as possible. Make as many columns as the number of the root to be extracted; then, having found the first quotient figure, put it in the first column; add the same number to it; then multiply the sum obtained, placing the product in the second column. Continue this course, viz.: add and multiply, add and multiply, decreasing one column each time, till you have only one to make in the first column, which will be the same number of additions as the root to be extracted. Then add 0 to the first column, 00 to the second, 000 to the third, 0000 to the fourth, and bring down the next period in the fifth. Then find the probable number of times that the fourth column will be contained in the fifth; put this in the quotient, and also under the first column, and add and multiply as before. This method was discovered by the late Mr. Horner, M.P., and is applicable to all roots. Your correspondent will find all particulars in "Kavanagh's Arithmetic."—Romeo.

Find the fifth root of 5153632.

				5153632 (22
2	4	8	16	32
2	8	24	64	1953632
4×2	12×2	32×2	800000	
2	12	48	176816	
6×2	24×2	80000	976816×2	1953632
2	16	8408		
8×2	4000	88408		
2	204			
100	4204			
2				
102×2				

Notice.—The necessities of space oblige us to defer solutions of problems till next week.—Ed.

Our Whist Column.

By "Five of Clubs."

THE PENULTIMATE.

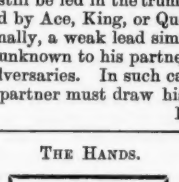
Sir,—I send you a game for young players, illustrative of the above heading. Playing the penultimate is, leading the lowest but one in suits of more than four cards, which are not headed by the Ace, or do not contain two commanding honours or strong sequences, &c. This mode of play is, practically, the invention of Cavendish, and is the logical extension of the lead of the lowest but one, in suits containing intermediate sequences. I look upon the invention as little inferior, in its means of conveying information (and its consequent influence upon the game), to the convention which requires a player to return his lowest in suits of four cards, or his highest in a suit of three. In point of fact, in some cases it conveys more rapid information, for the lead from five cards may be, and often is, declared in the second round of the suit. And where the dealer, being happily possessed of five trumps, is forced before trumps are led, and trumps with the penultimate, the information of a minimum of five trumps is conveyed at once. I ought, perhaps, to state that, unless there are special reasons to the contrary, the lowest but one should still be led in the trump suit, notwithstanding that it might be headed by Ace, King, or Queen, with small cards. It is true that, occasionally, a weak lead simulates a lead from five or more; for a player, unknown to his partner, may have his strong suit attacked by the adversaries. In such cases, where the lead is not an original lead, a partner must draw his inferences with more caution.

Frederick H. Lewis.

THE HANDS.

A.
Clubs—4, 3, 2.
Hearts—K, Kn, 4, 3, 2.
Spades—Kn, 8, 3.
Diamonds—8, 6.

B.
Clubs—Kn, 10, 9, 8.
Hearts—A, Q, 9, 6.
Spades—A, 5, 4.
Diamonds—K, 4.



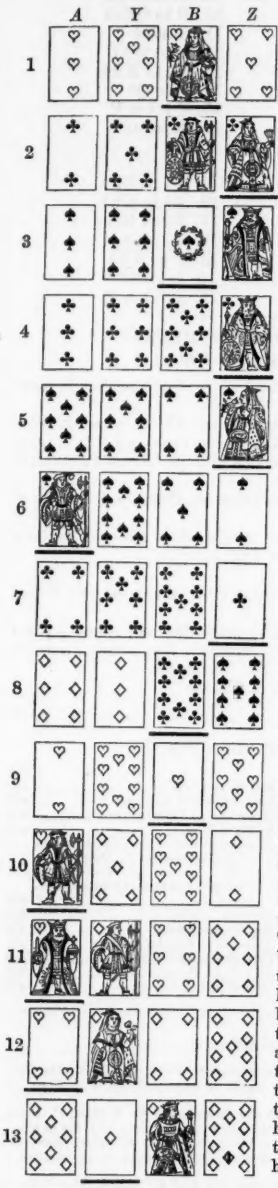
Y.
Clubs—7, 6, 5.
Hearts—10, 7.
Spades—10, 7, 6.
Diamonds—A, Q, Kn, 5, 3.

Z.
Clubs—A, K, Q.
Hearts—8, 5.
Spades—K, Q, 9, 2.
Diamonds—10, 9, 7, 2.

Score:—Three all.

THE GAME.

NOTE.—The underlined card wins trick, and card below it leads next.



REMARKS AND INFERENCES.

1.—*A* leads the lowest but one. *B* wins with the Queen and observes that the two does not fall in the trick. Either one of the players is calling for trumps, or *A* is leading from five or more. If from more than five, the suit will be trumped next round. Having a sequence, he leads the head of it.

2.—*B* knows now that the adversaries are two by honours. *Z*, having turned up the King, properly heads the trick with the Queen.

3.—*Z* plays his best suit. *B* winning the trick continues the trump lead. He knows that he must make two by cards to win the game. He properly leads the eight, to show his partner the strength of his sequence.

4.—*Z* continues his suit.

5.—*Z* plays another Spade in the hope of finding the Knave with *Y*.

6.—*A* knows now that the Ace will fall, and that *B* will, probably, remain with the last trump. Of course, there is a chance of *Z* having the seven.

7.—*Z* is now in a difficulty. He knows he can force the remaining trump, but he knows also that *B* has led trump upon the Heart suit. *Y* may, however, have an honour in Hearts, and it is better in any event, as far as the Diamonds are concerned, that *Y* should be fourth player. *Y* discards a Diamond to the 13th Spade, though not of much use now, as the only trump is forced from *B*. The rest of the game plays itself, but if the young player will play the game over, and suppose *B* to have led three rounds of Hearts before leading trump, he will find that *Y* and *Z* will make six tricks and win by their honours. If the two had been led instead of the three, *B*, although he might nevertheless have led trumps, would have had no means of judging that the entire suit was between him and *A*.

ERRATA.—It is, perhaps, hardly necessary to explain that in the vesy easy Double Dummy problem given last week, the partners A and B, not Z and Y, are to save and win the game. The correction was made in the proof sheets; but, in making a more important change ("Mogul's letter had been put last, instead of in its proper position), the compositor overlooked this one. A similar remark applies to the word "small," p. 416, 2nd col., l. 22. Clay abhorred "false" cards, not small ones. Whist players will recall Cavour's *mot:* when some one complained, as bad players always do, of constant ill-luck at whist, he replied, "C'est, mon ami, que vous n'avez pas assez de respect pour les petites cartes."—FIVE OF CLUBS.

ALL THE TRUMPS IN ONE HAND.—In *All the Year Round* for October, 1876, page 77, two instances are given of a player being dealt the entire trumps ["having dealt himself," it should be]. Very recently, I myself held all the trumps but two (five and six) in a hand at whist.—R. E. P.

A CORRESPONDENT asks how to score at Short Whist, not finding Clay's account clear. If he would say what seems to him obscure, we will endeavour to explain.—FIVE OF CLUBS.

J. MACKENZIE regrets to see KNOWLEDGE *wasted*, advising how best to play Whist, "a game depending so much on mere chance." *Does* it depend on mere chance? Cavendish has settled that question pretty decisively. The element of chance is eliminated in the long run, and good play tells as certainly at Whist as it does at Chess. The way chance works is to make Whist a capital moral as well as mental exercise. The player who, having a bad hand, steadily does all he can while yet a chance remains, and perhaps retrieves a game which another might have thrown up as hopeless, has had a useful exercise and set a good example.—FIVE OF CLUBS.

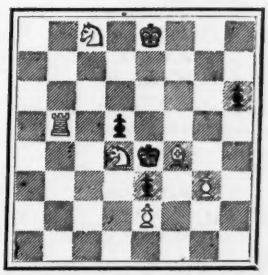
G. THOMPSON'S second letter, relating to B.'s lead in problem 1, somehow escaped the editor's notice until now, and has only just reached me. He points out that in a somewhat similar case, in Cavendish's fifth Hand, leader's partner prefers returning his partner's lead, to leading from a hand headed by a tenace (A, Q, and two small ones.) The play in these hands is not intended to guide Whist players, though Cavendish does not call attention to every error in the play. In Hand V, Cavendish calls attention to the importance of returning the best of two cards, without saying whether B would not have done better to show his best suit. There is, however, an important difference between the two cases. In Problem I, B. has length in trumps; in Cavendish's Hand V, B. has not. Again, in hand V, B. can return a good strengthening card. The ten of Clubs, in Problem I., played with the certainty that Y (fourth player) holds the Queen, and that Z (second player) is weak in Clubs, could scarcely be considered a strengthening card at all. The words to which I referred (which Mr. Thompson could not identify, having a different edition) are these:—"Many players will not lead from a strong suit if headed by a tenace; preferring, for instance, to lead from ten, nine, three, to Ace, Queen, four, two. They argue that by holding up the Ace, Queen suit, they stand a better chance of catching the King. So far they are right; but they purchase this advantage too dearly; for the probable loss from leading the weak suit may be taken as greater than the probable gain fron holding up the tenace." Mr. Thompson argues, however, that the case comes under Cavendish's advice about returning your partner's suit rather than your own, with weak or only moderately strong suits, which you open to a disadvantage. Does Mr. Thompson regard Ace, Queen, to four, as only a moderately strong suit? I should call this great strength. Cavendish refers to such a suit as Queen to four, in some cases King to four, or Queen, Knave, two small ones. Even in such cases as these, if you have strong cards in the adversaries' suits, it is better to show your own. FIVE OF CLUBS.

Our Chess Column.

PROBLEM No 25.

BY LEONARD P. REES.

BLACK



WHITE

White to play, and mate in three moves.

THE GIUOCO PIANO.

THIS form of the Knight's opening, called by the Italians the slow game, arises if, instead of moving his Kt. to B.3 on his third move, Black should play B. to B.4., *i.e.*,

1. P. to K.4. / P. to K.4. 2. Kt. to K.B.3. / Kt. to Q.B.3. 3. B. to B.4. / B. to B.4.

White has now four moves at his disposal, viz., 4. P. to B.3

4. P. to Q.3. 4. Kt. to B.3. 4. Castles. All these moves are safe, but slow in their effect. Modern players incline towards a more incisive line of play.

If White plays 4. P. to B.3., with the intention of playing P. to Q.4., when he would obtain a strong centre, the following play would result:—

4.	P. to B.3. / Kt. to B.3.	5.	P. to Q.4. / P. takes P.	6.	P. takes P. / B. to Kt. 5 (ch.)
7.	B. to Q.2. / B. takes B. (ch.)	8.	Q.Kt. takes B. / P. to Q.4, or (A.)	9.	P. takes P. / K.Kt. takes P.
10.	Q. to Kt.3. / Q.Kt. to K.2.	11.	Castles. / Castles.	12.	Q.R. to K.sq. / P. to Q.B.3.
13.	Kt. to K.5. / Q. to Kt.3.	even game.			
(A.) 8.	/ Kt. takes K.P.	9.	Kt. takes Kt. / P. to Q.4.	10.	B. takes P. or (*A.*) / Q. takes B.
11.	Castles. / B. to Kt.5.	12.	Kt. to B.3. / B. takes Kt.	Black having a slight superiority.	

(*A.*) Players of an attacking style might play 10. K.Kt. to Kt.5., then 10. / P. takes B. 11. Q. to R.5. / P. to K.Kt.3. 12. Q. to R.6. / Q. takes P. 13. Q.R. to Q.sq., followed by 14. Castles with a strong attack.

Instead of 9. Kt. takes Kt., White may also play 9. P. to Q.5., a move adopted by Mr. Steel, of Calcutta. This move cramps Black's game very much, and unless he carefully opposes it, he will get a bad game. Black might continue, 9. Kt. takes Kt. 10. Q. takes Kt. / Kt. to K.2. 11. P. to Q.6. / P. takes P. 12. Q. takes P. / Kt. to B.4. 13. Q. to Q.5. / Kt. to R.3. Castles Q.R.

It would obviously be bad to play 9. Kt. to R.4., as Black would then most likely lose this piece by P. to Q.Kt.4. being eventually played. Checking with the Queen on Black's tenth move would also be bad, as White would play K. to B.sq. On the whole, we think the move 9. P. to Q.5. is sound, and ought to obtain at least a drawn game.

After 4. P. to B.3. / Kt. to Kt.3, White might also continue with 5. P. to Q.3, a move favoured and played often by Mr. Blackburne, who then proceeded to bring his Q.Kt. over to his King's side. Should Black Castle too hastily on his King's side, White would at once proceed with an attack, by advancing P. to K.R.4., *i.e.*,

5.	P. to Q.3. / P. to Q.3.	6.	B. to K.3. / B. to Kt.3.	7.	Q.Kt. to Q.2. / Kt. to K.2.
8.	Kt. to B.sq. / P. to B.3.	9.	Kt. to Kt.3. / P. to K.R.3.	10.	Q. to K.2. / P. to Kt.4.
11.	Castles Q.R. / Kt. to Kt.3.	12.	P. to Q.4.* / Q. to K.2.		

If, on his sixth move, White proceeds with P. to K.5., Black ought to obtain the better game, *i.e.*,

1.	P. to K.4. / P. to K.4.	2.	Kt. to K.B.3. / Kt. to Q.B.3.	3.	B. to B.4. / B. to B.4.
4.	P. to B.3. / Kt. to B.3.	5.	P. to Q.4. / P. takes P.	6.	P. to K.5.

Black's best reply to this is 6. P. to Q.4. If, now, White replies with 7. P. takes Kt., a likely-looking move, Black will have a very good and valid defence, *i.e.*,

7.	P. takes Kt. / P. takes B.	8.	P. takes Kt.P. / R. to Kt.sq.	9.	B. to Kt.5. / P. to B.3.
10.	Q. to K.2.(ch.) / Q. to K.2.	11.	B. takes P. / Q. takes Q.(ch.)	12.	K. takes Q. / P. to Q.6.(ch.),

with the advantage.

Better than 7. P. takes Kt., is B. to Q.Kt.5., but even then Black would get the better game, *i.e.*,

7.	B. to Q.Kt.5. / Kt. to K.5.	8.	P. takes P. / B. to Kt.3.	9.	B. takes Kt.(ch.) / P. takes B.
10.	Kt. to B.3. / Castles.	11.	Castles. / B. to Kt.5.	12.	B. to K.3. / Kt. takes Kt.
13.	P. takes Kt. / P. to K.B.3.	14.	P. to Q.R.4. / P. takes P.	15.	P. to R.5. / P. to K.5.,

and Black has the better game.

(*To be continued.*)

* As played in the match by Blackburne against Zukertort.

GAMES BY CORRESPONDENCE.—(*Continued from p. 415.*)

GAME I.

Chief Editor.	Chess Editor.
	10. B to QKt3
11. Q to K2	11. Castles
12. P to KR4	12. P to QR3
13. P to KKt8	13. Kt to KB4
14. R to KR3	14. P to Q5
15. Kt to Kt5	15. P takes P
16. B takes Kt	16. R takes B
17. P to KKt4	

GAME II.

	9. Kt to KR3
10. Kt to Q2	10. Kt to KB4
11. P to QB3	11. Kt to Kt sq
12. Kt to QB4	12. B to B3
13. K to B2	13. P to KR3
14. B to Q3	14. Q to B2
15. Kt to QR5	15. P takes P

ANSWERS TO CORRESPONDENTS.

*** *Please address Chess-Editor.*

J. F. B.—You have taken the correct view of the subject in your last letter to us.

Geo. Bell.—Only Pawns "can take Pawns" in passing. For instance, place White Pawn on K5, Black Pawn on Q2; if now Black moves P to Q4, White's Pawn can take the Black Pawn as if it had only moved to Q3.

Clare.—Cook's "Synopsis," 3rd edition, or Gossip's "Theory of the Openings."

J. Park.—Game well known; also published in Howard Taylor's "Chess Brilliants." 2. From about 1840 to 1850.

J. Hall.—Thanks.

D. See.—We should be going beyond the scope of our Chess Column.

John Griffith.—In Castling the King only must not pass a square commanded by a hostile piece. 1. Wormald has about 74 or 76. 2. It is quite a different thing. 3. No. 4. 3s. 6d.

Letters received from J. Licence, J. Watson.

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